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THE PSYCHOLOGICAL REVIEW.

SOCIAL PSYCHOLOGY AND SOCIOLOGY.¹

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I presume that every one will be disposed to admit that scientific investigation is confronted at its very origin and in every field of reality, with three fundamental and preliminary questions: (1) Where must research commence? (2) to what group of phenomena must it be confined? and (3) by what methods must it be conducted? This is what we may define, in terms of Kantian epistemology, as the 'critical' problem. In physics and biology this stage of critical determination of the field of inquiry has long since been overcome. In those two great departments of knowledge every single science has well-defined boundaries, and the phenomenon or the group of phenomena studied by each is typically differentiated, so as to constitute a very distinct and well-defined object of research. But with sociology, on the contrary, the problem is far from being solved. Even the most enthusiastic believer in the future of the would-be science, unless he be utterly incapable of impartial judgment, cannot but acknowledge the fact that we have nothing more than a statement of the problem. It is true that a certain concordance of tendencies which might eventually lead to an agreement on some important points is making its way out of the chaotic intermingling of theories. But it is none the less evident that the province of sociology is as yet very roughly and vaguely determined, at least in the minds of those, we may call them 'irréguliers de la philosophie,' who form the bulk of the sociological confraternity.

¹An address to the Princeton Psychological Seminary, March 31, 1898.

Under these circumstances, while sociology, as a French writer recently said, 'se cherche encore,' and while the strongest efforts of the authentic sociologists are strenuously bent in the direction, above all, of defining the proper field of the science, a new element of uncertainty and disorganization seems to come from the tremendous growth of a branch of psychology dealing with particular aspects of the social fact and hence called social psychology. The wide interest aroused by these new researches of psychologists has been followed by misconceptions of various character. Some have supposed the new branch of psychology to be destined to swallow up sociology. A competent psychologist has expressed the opinion that 'social psychology' only could succeed where sociology has so completely failed, namely, in giving the explanation of the social enigma.¹ Some have considered 'social psychology' as a new name for sociology, as a synonym; nay, as a rhetorical substitute. Such is the case with those pseudo-scientists who have talked to satiety of 'collective' psychology àpropos of the crowd, referring the word 'psychology' to the 'social' mind metaphorically assimilated to the 'individual' mind. Amid such confusion it is no wonder that the discredit attached to sociology by its impotence to crystallize into a concrete and coherent body of science is increased and that it falls back on the alleged new branch of psychology. People ask, What is this so-called 'social' psychology? Is it not a new label for selling off the damaged stock of sociological generalities? And how can a 'social' psychology exist independently of sociology, since we are taught that sociology itself cannot but be grounded on psychology, every attempt to explain society in terms of a biological interpretation having completely failed?

If, then, we have not to renounce entirely the hope of disclosing the world of social phenomena to scientific investigation, we are confronted, at this very moment, with the urgent task of solving the critical problem of social science. Any contribution, however modest it may be, to the determination of the proper field of sociological inquiry will be, in the present precarious conditions of sociology, a distinct contribution to its

¹ Professor J. H. Tufts, *PSYCHOLOGICAL REVIEW*, November, 1897.

organization into a science. In the light of this main problem, the question of the relationship between 'social' psychology and 'sociology' presents itself as a very interesting one. And it will be the object of this summary sketch, in broad outlines to determine, and rather with the purpose of preparing the elements for further deeper discussion :

(1) Whether the psychologist, as such, is at all concerned with the social phenomenon ; (2) if so, what is the specific object of 'social' psychology as opposed to sociology.

I.

We cannot hold a comprehensive view of the former of the two questions proposed, without calling to mind the typical standpoint of psychological investigation. "Psychology," says Dr. James Ward,¹ and I could not express my thought better, "never transcends the limits of the individual. * * * Of all the facts with which he deals, the psychologist may truly say that their *esse* is *percipi*, inasmuch as all his facts are facts of presentation, are ideas in Locke's sense or objects which imply a subject. Before we became conscious there was no world for us, should our consciousness cease, the world for us ceases too, had we been born blind, the world would for us have had no color, if deaf, it would have had no sounds, if idiotic, it would have had no meaning.* * * By whatever methods, from whatever sources the facts of psychology are ascertained, they must—to have a psychological import—be regarded as having place in or as being part of *some one's consciousness*. In this sense, *i. e.*, as presented to an individual, 'the whole choir of heaven and furniture of earth' may belong to psychology, but otherwise they are psychological non-entities." Admitting this conception of psychology to be the only comprehensive one of the distinctive and really characteristic feature of the science, we must, however, recall the fact that man is a gregarious animal ; that his gregariousness, far from being a peculiarity of his own nature, is, on the contrary, the manifestation of a more general fact shown by the distribution of animals over the surface of the earth. "With

¹ In *Encyclopædia Britannica*, Ninth Edition, Vol. XX., art. ' Psychology, p. 38.

few exceptions, living beings are disposed in groups which here are loose and scattered, and there are massed in dense aggregations."¹ A man living in isolation is a pure 'a priori' creation of the 'pure' reason. The gregariousness of man being recognized as a characteristic of his animal nature, it follows that some degree of aggregation, based on merely organic relations, precedes the appearance of both mind and society, and is the crucible where these two great energies of life are generated. At this stage of development, which might be termed 'pre-human,' thought has not yet found its way out of the darkness of the animal consciousness, while society, in the proper meaning of the word, has not yet evolved from mere propinquity and material contact. But it is just at this period of the history of our species, when man is barely differentiated from his animal ancestors, that we must seek for the earlier manifestations of both the mental and the social fact. These two orders of phenomena spring up together from the interaction between the group and the individual, and they are so inextricably interwoven at their origin that any attempt to ascertain the priority in time of either of them would prove utterly aimless. Thus, while the development of mind seems to be conditioned by a certain degree of aggregation, the evolution of society out of the primitive group appears to be, in the main lines, connected with the increasing intensity of the mental reactions. Through human history the two facts of mind and society run in a linked way which strongly reminds us of the connection between mental facts and nervous changes ascertained by the psychologist.

If this be true, if we cannot conceive men as living in isolation, if some degree of aggregation shows itself at the very dawn of human life, and appears to be incident to the most elementary manifestations of human intelligence, then we cannot explain the mental development, either in the species or in the individual, without taking into account the action of the social environment upon the individual, and the intricate and subtle interchange of elements between the two factors. We are thus

¹ See F. H. Giddings, 'The Principles of Sociology,' New York, Macmillan, 1896, p. 79.

led to consider the first way through which the social phenomenon might fall within the range of psychological investigation—namely, the possibility of studying the action of the social milieu upon the development of the individual mind. But, from this point of view, we cannot give the qualification of ‘social’ to the psychological research, because, by so doing, the whole of psychology would be absorbed into the so-called ‘social’ psychology. If the individual mind cannot be separated in its growth from the social milieu in which it is rooted, and from which it draws its most vital nourishment; if, as Professor Baldwin so perspicuously says, “a man is a social outcome rather than a social unit,”¹ then it becomes evident that psychology is, at every step, confronted with the task of ascertaining the psychological bearing of the sociological facts, whether the object of the research be the phylogenetic development of consciousness (race psychology in Professor Baldwin’s terminology) or whether it be the ontogenetic development of mind (infant psychology). It is safe to assert that a sociological interpretation of mental development is coextensive with the proper field of ‘genetic’ psychology as opposed to ‘descriptive’ psychology.² Thus the word ‘social,’ when chosen to qualify this kind of psychological investigation, which

¹J. M. Baldwin, ‘Social and Ethical Interpretations in Mental Development,’ New York, Macmillan, 1897, p. 87.

²In the course of the interesting discussion which followed the reading of the present paper, Dr. Ormond, the distinguished Professor of Princeton University, made the remark, that the normal adult psychology is not only ‘descriptive’ but also ‘analytic.’ It gives not a merely superficial view of the mental processes, but goes deeper, in so far as it seeks to extricate out of the complicated intermingling of the mental functions, the ultimate elements, the typically essential elements of the mental fact; thus leading to a more comprehensive view of the mental process, in its entirety. According to Dr. Ormond, psychology should be naturally distinguished into three separate branches, (1) the genetic, (2) the descriptive, (3) the analytic. But in my thought, as expressed in my paper, the proper field of ‘descriptive’ psychology was meant as coextensive with just that assigned to ‘analytic’ psychology by Dr. Ormond. The question then becomes chiefly one of words. I have no difficulty in admitting that the proposed denomination (analytic) is clearer and more comprehensive than that of ‘descriptive,’ but by adopting the former I would eliminate the latter. Description cannot be separated from analysis. Moreover, description is necessarily the outcome of analysis. Hence psychology ought to be divided only into ‘genetic’ and ‘analytic,’ the relationship of these two branches of mental science being analogous to that of embryology to physiology (brain embryology and brain physiology).

might appropriately be termed 'mental embryology,' becomes a pleonasm destined to generate vagueness and confusion.

But there is yet another way of looking at the social fact from the typically individualistic standpoint of psychology.

At a certain stage of that process of interchange between the individual and his fellows that Professor Baldwin defines 'the Dialectic of Personal Growth,'¹ we may detect the appearance of a peculiar state of consciousness brought about by the rise of the conception of man 'as a multiplication of particular men *like myself*.'² At that stage of personal development we find that man, as Professor Baldwin puts it, "thinks of the other, the alter, as his *socius*, just as he thinks of himself as the other's *socius*."³ This particular state of the individual mind, which may be said to constitute the subjective element of the social intercourse, is the starting point of a series of complicated reactions which gradually lead to the rise of the ethical sense, the highest socializing force in the individual. Now, if psychology is to be concerned, according to the definition of Professor Ladd, endorsed by Professor James, with the 'description and explanation of states of consciousness as such,'⁴ there can be no possibility of contesting that psychology is confronted with the task of accounting for those peculiar states of like-mindedness determined in the individual by the presence of and the contact with his fellows. But, if it be true, as Professor Baldwin has so brilliantly demonstrated, "that what the person thinks as himself is a pole or terminus at one end of an opposition, in the sense of personality generally, and that the other pole or terminus is the thought he has of the other person, the alter * * * if it be impossible to isolate his thought of himself at any time and say that in thinking of himself he is not essentially thinking of the alter also,⁵ * * * if the thought

¹ Op. cit., pp. 7-9.

² J. M. Baldwin, *Handbook of Psychology*, New York, Henry Holt & Co., 1890-92, Vol. II, p. 193.

³ Soc. and Eth. Interp., p. 24.

⁴ W. James, *Psychology (Briefer Course)*, New York, Henry Holt & Co., 1895, p. 1.

⁵ Soc. and Eth. Int., pp. 9, 10.

of self is, in the main, as to its character as a personal self filled up with the thought of others, distributed variously as individuals, and the thought of others, as persons, is mainly filled up with the thought of self * * *¹; if, in short, *the real self is the bipolar self, the social self, the socius*,² then, again, the would-be 'social psychology' appears to be coextensive with the doctrine of personal growth in which 'genetic' psychology substantially concretes itself. Stated in the foregoing terms, the question becomes, largely, one of denomination. The 'social' state of mind is one of a series of phenomena shown by the mental development of the individual, and must therefore be studied, like every other growing element of the mind, in connection with the various terms of the whole series. There would be no harm in the use of the new name 'social psychology' for such an inquiry, on the condition, however, that it be distinctly understood that the so-called 'social psychology' is nothing more than a chapter of 'genetic psychology.' In inquiring into the origin and growth of both the 'social' and 'ethical' feeling, the psychologist does not go beyond the typically individualistic standpoint of his science, and therefore finds himself only half way on the road leading to the interpretation of the social phenomenon as such.

II.

Such an inquiry was not possible so long as psychology was bound to consider the mind as a fixed substance with fixed attributes. The 'rational' psychology, in setting up the soul as an absolute being, with certain faculties of its own, by which the several mental activities were explained, almost without reference to the peculiarities of the world with which they dealt, was absolutely unable even to state the question of the rise and development of both the social and ethical feelings. As long as such a conception of the mind was dominant, the assumption of a 'social' feeling, as a datum of ethics and social philosophy, never could have led to the constitution of a 'social psychology' in the present acceptance of the phrase.

¹ Ibid., p. 12.

² Ibid., p. 24.

In tracing back the history of 'social psychology' we cannot, therefore, take into consideration the work of the English moralists of the eighteenth century, known in the History of Philosophy under the name of 'sentimentalists,' whose altogether remarkable effort finds its highest embodiment in Adam Smith's 'Theory of the Moral Sentiments.' There we have, it is true, an attempt to define what we may term the psychological elements of the ethical phenomenon. But the analysis of the alleged 'social instinct' is not carried very far, while 'sympathy' and 'sociability' are thought of as dogmatic presuppositions, based on vague intuitions. Nor can we look for anything approaching a beginning of 'social psychology' in those attempts to explain the historical mystery and to detect some regularity in the incoherent pell-mell of the social facts, an attempt which, under the name of the Philosophy of History, from Vico and Herder down to Hegel, prepares for sociology as a first endeavor towards the scientific observation and classification of social facts. There also the 'socius' remains unexplained, and the 'social' or the 'ethical' feeling is the undisussed datum, furnishing a common basis for the various interpretations of human history.

I hardly need to say, that in spite of its name, the 'Völkerpsychologie' of Lazarus and Steinthal has nothing in common with 'social psychology.' The identity of the denomination must not mislead us. The 'Völkerpsychologie' is indeed a confluence where end all the brooks and torrents of the Post-Kantian Idealism. The word 'Psychologie' is here taken only in a metaphoric, figurative sense, with reference to a 'collective mind,' whose conditions of life and development Lazarus aims solely to ascertain. The 'Völkerpsychologie' is a new name for the Philosophy of History. The problems Lazarus deals with, whenever he attempts to determine the proper field of the new science, are undoubtedly sociological problems, as, for instance, the elements and character of the 'social mind' or the action of the genius on human development. Here the question at issue is not the rise and growth of a 'social' state of mind, in the individual, but rather the determination of the law of mutual interaction of individuals in association.

But if it be true that 'social psychology' never could have arisen before the evolutionary idea had reversed the old scholastic conception of the soul, it is also true that 'social psychology' has been rendered possible only by the discovery of the way through which one brain acts upon another brain, in society. It would not have sufficed to know that the 'social' and 'ethical' feelings are growing, developing activities, similar in character to every other element of the mind. The knowledge of the true way of propagation of ideas from one individual to another, of the manner in which the suggestions from the social *milieu* are transmitted to the individual, was indeed the only possible departure for an analysis of the rise and growth of the 'social' state of mind.

As I have remarked elsewhere,¹ the idea of the transmissibility of thought and of its modifying power upon the social environment is the ripest fruit of that same evolutionary philosophy that had begun by so strongly emphasizing the predominance of physical and biological factors in social development. In the midst of the naturalistic 'tourmente,' while the metaphor of the 'social organism' was exerting so disturbing an influence upon the progress of social science, experimental psychology and mental pathology were beginning to reveal the miracles of hypnotism and suggestion. It is through hypnotism that Gabriel Tarde succeeded in discovering imitation, which appeared to his original mind to be the way of propagation of thought and the real path by which the 'social' state of consciousness is determined in the individual. It is incontestable that the possibility of what is now called 'social psychology' has been created by Tarde's discovery of the elementary force that brings about the gradual building up of the sense of self, the process—as Professor Baldwin would say—of 'dialectical growth of personality.' The book on 'Les Lois de l'Imitation' ('a work of genius,' as a competent judge, Professor William James, has defined it)² has been undoubtedly the powerful fer-

¹ See my paper on 'Tarde's Sociological Theories,' in *Political Science Quarterly*, September, 1897.

² W. James, 'The Will to Believe,' Essays in Popular Philosophy, Longmans Green & Co., 1897, p. 261.

ment destined to coagulate, as it were, the scattered elements of 'social psychology.' All the work that is now being done in the direction of ascertaining the rise and growth of the 'social state of mind' is partly an expansion, partly a verification, and only on minor points a correction, of Tarde's intuition of the law of social dynamism. Let me sum up my view of the origin of the so-called 'social psychology' by an image borrowed from the science of life. Let me say that 'social psychology' grows out of the contact of two distinct nuclei of procreation: (1) The evolutionary conception of the mind; (2) The conception of the thought as a tremendous agency of social transformation and personal development. The theory of imitation is the fertilized ovum in which the fusion of the two pronuclei appears to have been already accomplished.

III.

If, then, 'social psychology' is to be conceived as a mere name for a chapter of 'genetic' psychology, dealing with the peculiar emotions and instinctive or impulsive reactions which are, in the individual, consequent, first, upon the fact of aggregation, and secondly, in a more advanced stage of social development, upon the fact of association, we can easily perceive what position it bears to sociology.

What is sociology? The answer to such a question does not seem easy at first. Yet, on reflecting well, we may detect a general tendency finding its way out of the deceptive proliferation of social theories. It is a tendency which seems to be common to the best and most authentic sociologists, and which might lead to an agreement on the basis of the conception of the science masterfully outlined by Professor F. H. Giddings, in his admirable chapter of his 'Principles of Sociology' dealing with the question of the 'province of the science.'¹ According to this conception, sociology is not a new name for the group of the *special* social sciences, it is not an encyclopedic science of society coextensive with the entire field of the special social sciences, but it is, on the contrary, a science by itself, with a

¹ F. H. Giddings, 'The Principles of Sociology,' New York, Macmillan, 1897, pp. 21-51.

specific object and a function of its own. "Whenever," says Professor Giddings, "phenomena belonging to a single class, and, therefore, properly the subject-matter of a single science, are so numerous and so complicated that no one investigator can hope to become acquainted with them all, they will be divided among many particular sciences; yet, there may be a general science of the phenomena in their entirety, as a class, on one condition, namely, the general science must deal with attributes of the class that are common to all of its subclasses and not with the particular attributes of any subclass. Such common attributes are elementary. General principles are fundamental. A general science, therefore, is a science of elements and first principles."¹ Sociology is, then, the general science of social phenomena, *i. e.*, the science whose object is to determine the elements which are common to all the different classes of social phenomena. It is 'the science of social elements and first principles,'² and bears to the special social sciences the same position that biology holds to the special sciences of life.³

But what are those general attributes of the social fact that are studied by sociology? In other words, what is the typical character of the 'social' phenomenon as opposed to the 'mental' which falls within the limits of psychological investigation?

Here we find ourselves on very slippery ground. It seems nearly impossible to answer the question among the contradictory theories which have been pullulating on the subject, each claiming to detect the true character. the only really essential character of the social phenomenon. Here the leading string is undoubtedly the Tardian intuition of the inter-cerebral action which is, either consciously or unconsciously, presupposed by every possible interpretation of the social fact. We may, for the purpose of present inquiry, somewhat roughly define the social phenomenon as the phenomenon of the mutual interaction of minds in aggregation.

If such a conception of sociology and of the social phenom-

¹ Ibid., p. 31.

² Ibid., p. 33.

³ Ibid., p. 32.

enon be accepted, we may easily draw a sharp line of demarcation between 'social psychology' and sociology. Social psychology is concerned with *the genesis of that particular state of consciousness which is consequent in the individual upon the presence of and the contact with his fellows*. Sociology studies *the phenomena that are consequent upon that particular state of consciousness, the social state of mind*. It follows that social psychology goes beyond its proper field of inquiry whenever it seeks to determine the way in which the 'socius' reacts upon its social environment, thus becoming an active factor of modification of the social milieu itself; while sociology goes equally beyond its boundaries when it takes up the study of the genesis of the 'social' or 'ethical' feeling. These must be data furnished to sociology by social psychology. Sociology must accept them uncritically, in the same way that other sciences assume a fundamental datum unquestioningly, leaving it to the other parts of philosophy to ascertain and scrutinize their significance and truth. This is the only way of adopting in sociology the general point of view of 'natural science,' whose adoption has so greatly contributed to the recent progress of psychology and to its separation from the metaphysical womb.

But we can go further in determining the limits of the respective fields of investigation of social psychology and sociology. Sociology, we have said, studies the phenomena that are consequent upon one state of consciousness, the social state of mind. Now, all these phenomena have a common character, which we may detect even in their most elementary manifestations. If we look to the development of the child for light upon the social movements of his nature, we find that while the child "imitates and follows all examples set around him," he shows, on the other hand, as Professor Baldwin remarks, a certain bold aggressiveness, inventiveness, a showing off in the use he makes of the things he learns. We may suppose the persons about him divided roughly into two classes: those from whom he learns and those on whom he practices.¹ The first are those in whom he recognizes new elements of personal suggestion not yet accommodated to, and whom he therefore imitates.

¹ Eth. and Soc. Int., p. 17.

The others are those whose features he has already mastered, who do all he can do, and who therefore become for him an instrument of self-assertion and a means of testing his superior activities.¹ This reaction of the individual mind upon its social milieu discloses the boundaries of the psychological phenomenon and brings us directly to the heart of the social fact. Inasmuch as the individual, after having been a recipient for examples set by others, becomes in his turn a centre of propagation for new thoughts of his own, then he may be said to contribute to the production of the social phenomenon as such. The notion of an energy striving to find its way out of the organism in which it is incorporated is indeed essential to the complete determination of the typical element of the social fact. This view of the difference between the psychological and the sociological phenomena has been masterfully expressed by Professor Giddings in the following passage of his book: "In both biology and psychology phenomena within the organism are regarded as effects, and relations in the environment as causes. On turning to social phenomena it is discovered that activities within the organism have become conspicuous as causes. They have created a wonderful structure of external relationships, and have even modified the fauna and the flora and the surface of the earth within their environment." Thus sociology is the science of "the constructive evolution of a social medium through which the adaptations of life and its environment become reciprocal."² Here we find the limpid, clear formula which we must retain as the most perfect determination of the proper character of the sociological fact. While in social psychology a 'phenomenon within the organism,' the social state of mind (be it called the notion of the 'socius' in Professor Baldwin's terminology or the 'consciousness of kind' in that of Professor Giddings), appears to be an effect of causes lying in the social environment, whence flows the stream of examples toward the receptive individual, in sociology an 'activity within the organism,' inventiveness, appears as a cause of transformation of the social milieu by creating new centres of propagation of examples.

¹ Ibid., pp. 18, 19.

² F. H. Giddings, op. cit., pp. 25, 26.

Society, according to this conception, can be compared to a network in which every knot is represented by an agency of elaboration of new thoughts and examples furnished by the inventive, aggressive, self-asserting side of every individual mind in aggregation. In Tardian terminology we may state our position by saying that social psychology is mainly concerned with facts of 'imitation,' while sociology is chiefly confronted with facts of 'invention.'¹

IV.

It would be an extremely interesting task to review the different groups of social phenomena with the purpose of ascertaining whether or not, in every field of social activity, the inventive energy of the leading men be the kernel from which the whole development of social institutions proceeds. It would probably be still more interesting to examine the ethical bearings of the conception of social life which I have just outlined. It seems evident that by identifying the typical character of the social fact with the reaction of the individual mind upon its environment, our doctrine discloses the boundaries of historical fatalism, and lifts us to a conception of social life in which is revived the Carlylian intuition of the modifying power of thought upon its environment. If we appear to ourselves as the artisans of our own destinies, by virtue of our thought and will, then, undoubtedly, life must appear to us as worth living. But I really do not dare to extend my discourse beyond the reasonable limits allotted to me by your kindness. Permit me to observe only that the conception of both social psychology and sociology that I have endeavored to outline in this rough sketch shows the possibility of eliminating the confusion I have pointed

¹This position includes the distinction acutely made by Professor Baldwin (*op. cit.*, pp. 475-478) between (*a*) the *matter or content* of social organization and (*b*) the *functional method or process* of organization of the social material. Invention, *i. e.*, thought, is the matter, while imitation is the process of organization in society. Without the material there would be, of course, no possibility of organization. Thence the assumption of invention as the only really typical element of the social fact. But this point will be more fully developed in my book on 'Social Psychology and Sociology,' to be published shortly by Alcan, Paris.

out, at the beginning, as an impending danger to the two sciences. To the psychologist who deems it possible to substitute for sociology the new all-invading branch of psychology, I will simply recall that social fact cannot be explained by tracing back the history of the growth of personality, since the appearance of the 'social' state of mind is only a stage of that growth, therefore an individual fact. The social phenomenon, as opposed to the psychological, begins only when the individual mind reacts upon its social environment, by setting new examples, thus becoming a new center for imitative propagation. On the other hand, I would remind the sociologist that inventiveness is not an explosion, breaking the continuity of the phenomenal series, but the result of an interaction of social elements brought in contact through an individual mind; and that the transmission of social suggestions, which is the necessary antecedent of invention, would be impossible without presupposing some degree of aggregation, and, consequently, an elementary state of the individual mind brought about by the presence of and the contact with his kind. The origin and growth of such a state of consciousness must necessarily fall beyond the range of sociological inquiry, because it is a phenomenon which takes place in the 'individual' mind, whose elements psychology alone aims to ascertain.

This line of demarcation being accepted, the possibility of an understanding between psychologists and sociologists will be substantially reached. Let me, in concluding, express the hope that the results of further scientific investigation in the field of social phenomena may still continue to bring proofs in support of that doctrine of society which, through many lines of development, grows out of Tarde's intuition. There we have a doctrine of society which I firmly believe to be the most apt to restore our faith in the efficiency of action and in the value of life.

PSYCHICAL RESEARCH AND COINCIDENCES.

BY PROFESSOR JAMES H. HYSLOP.

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From time immemorial coincidences have been objects of uncommon human interest and curiosity. The discovery of them still leads, as it always has led, to all sorts of superstitions. They survive to be remarked even by those who laugh at them. If a knife falls on the floor a stranger may be expected. Hundreds of such 'signs,' originating from the observation of chance coincidences, are at the constant command of the average man or woman, whether believed or ridiculed. Another class of coincidences which are more striking appeals to the instinct for special providences, mysterious meaning or supernatural explanation of some kind. They are often sufficiently striking and respectably authenticated to puzzle wise heads for a means to dislodge the impression of their real or possible causal significance. The collection and preservation of them by the Society for Psychical Research, no matter what we may think of them, has done much to strengthen the interest and belief in the possible meaning of such phenomena, especially when they take a certain form. The scientific or even quasi-scientific investigation of such things invests them with an importance that would not belong to them naturally, and that would make little impression upon the organized power of scientific opinion unless equally organized and sustained. But it is not necessary to warn scientific men against treating coincidences seriously. They are proof enough against that temptation.

There is, however, a complaint which I have to make against them. It is not for remissness in their allegiance to scientific method, but for an unnecessary failure to apply it as fully as it might be done in a field where the term 'coincidence' gives rise to very different illusions. There are 'coincidences'

and 'coincidences.' Not that I shall here beg any questions as to the important significance of any of them for the supernatural or for anything resembling it, but that some are undoubtedly more suggestive of the need of a causal explanation than others. Hence I shall distinguish for the purposes of this paper between two kinds of coincidences. The first I shall call *formal* or unsuggestive and the second *material* or suggestive coincidences. I intend no mysterious meaning or distinction by the terms 'formal' and 'material.' They represent only the difference between coincidences that are *mere* coincidences and coincidences that also have some identity of *content* more or less of a striking and suggestive character. The distinction is perhaps the same as that between what we call *casual* or chance coincidences and *causal* or significant coincidences. An illustration of the former kind is that of an unexpected meeting of two friends at a great distance from their usual habitat and without any previous knowledge of each other's movements. Of the second kind is perhaps the case of absolutely identical thoughts under circumstances which do not superficially explain the identity, but which may be traced to association awakened by some object having a common interest or connected with a common experience. In the first class belong all those conjunctions of things that do not involve the concerted or purposive action of the subjects experiencing them and not involving any such identity of content or adaptive fitness of several events to a single end. In the latter class belong all those coincidences that involve either concerted action or common known and unknown causes. The average scientific man, however, too often lumps all coincidences together indiscriminately, making the conception a hard-and-fast one and convertible with that of causeless connections. But there are traces of cowardice and equivocation in this attitude of mind or *a priori* method of treating phenomena that too often prevents the scientific man from recognizing in some coincidences a causal nexus of a very interesting kind, though not of the sort alleged by the supernaturalist and coincidence-mongers generally. Not to make the distinction, therefore, which I have made between the mere fact of coincidence and the

coincidence of content, is an error that leads to an unscientific treatment of such problems and prevents a search for obscure causes that are quite within the reach of normal psychology or recognized agencies.

In remarking this error to which the scientist often exposes himself I have in mind a defect of Parish's very able criticism of the Census of Hallucinations. I do not mean, however, to use this defect either as a defense of psychical research or as an impeachment of his method of criticism. On the contrary, no one can read that book without being convinced of its cogency and importance, if he did not know it before. In addition to this concession, the defect which I wish to remark is the failure to observe facts directly counter to dissociation and illusions of memory, and which would immensely have strengthened his verdict of 'not proven' against the supposition of supernormal agencies.

The first obvious defect of Parish's work is that there is no evidence of any *inside* study of the phenomena the conclusions from which he criticises. The second objection is that he risks the whole force of his criticism upon the suspicion of dissociation and illusions of memory, in which the responsibility for the defective nature of the cases reported falls upon the subject of the narrative, and not upon the receiver of it. But I wish here to contend that not only may there be cases in which the difficulty is not what Parish supposes, but that the really serious difficulties can often be found only by a careful study of the individual case. I mean here, of course, the study of the mental habits, beliefs and laws of association in the individual reporting a remarkable experience. Consequently I desire to show in this paper that there is a more important source of misconception than the subject's illusions of memory, and that can be discovered often only on the condition that we accept the deliverances of this faculty. Illusions of memory are of course a vantage ground for objection, but are neither the only ones nor the best ones. I shall show this after stating the facts upon which the conviction rests, and which have been gathered from a personal study of an individual case of some interest. The facts represent an extraordinary combination of apparitions and

apparent premonition in which a purely objective and superficial view would suggest a supernormal and perhaps spiritistic interpretation. Before making any comments or explanations I shall narrate the incidents in the order in which they occurred, and in which I obtained them. The subject of the experiences is one who has no prejudices in favor of such phenomena. On the contrary, the antipathy to anything like a spiritistic view of them is unyielding and marked by what the skeptic would regard as a very healthy disgust. The intelligence is sufficient to make the facts entirely acceptable, and, though some of them will not strike a scientific observer as of any serious interest, especially when thought of as isolated, yet the at least amusingly cumulative character of them and their distinct semblance to those experiences which so many people feel may be significant are striking enough to justify analysis and explanation, especially when this explanation exhibits a neglected source of misconception. The facts, then, are as follows:

The experiences to be here narrated are those of a lady whom I shall call Mrs. D. She is the same subject of whom I have reported a number of other interesting incidents in the Proceedings of the Society for Psychical Research. This fact will be useful to know if the reader wishes to study the whole group of phenomena coming from the same source. But the present group is wholly independent of the earlier cases.

Some time in July, 1897, Mrs. D. had a strong impression that some unusual 'burden' was going to fall upon the family. She could describe the feeling in no other way, and it will be noticed that the expression is a common one with religious minds, which often employ the term to denote a providential affliction. This meaning Mrs. D. gave to the term herself. But the feeling was too vague to identify with any past cause or any incident to be forecasted in the future. In stating the fact also it must be remembered that Mrs. D. was in good health, in fact, better than usual, as the phrase goes, though at no time does she have to complain of more than the indisposition of people who have the personal care of their children and the domestic work. Hence there was nothing in her physical condition that would suggest a clear physical cause of such a feeling, nor any

meaning that might deserve attention. I am not implying that there were no such causes, for there may have been conditions that a skilled physician would detect. But to the consciousness of the subject there was no indication of indisposition of any kind. In fact, she has answered all my inquiries on this point to the effect that her peculiar experiences always occur most frequently when her health is at its best, so far as her own judgment can determine. Throughout the whole period over which the present narrative extends her health was good. In the month of August this premonitory feeling repeated itself very frequently, and became so annoying that Mrs. D. mentioned it to her husband, who confirms her statement in regard to both facts, and hence supports the supposition that the location of the experience previous to its real or supposed fulfilment is not due to an illusion of memory. Finally, the feeling became so intense and persistent that Mrs. D., as is often the case with religious minds as deeply imbued with piety as is her own, sought relief in prayer. But though this resource had in her estimation been effective in other cases where it had been instigated, as might well be in a mind so sensitive to automatisms as is her own, yet the feeling could not be dismissed, and with a conviction that the affliction was not to be evaded she sought to cultivate the frame of mind suited to the endurance of the inevitable.

To make the matter clearer it is necessary to anticipate the sequel of the story, to which the incidents of the narrative are supposed to refer. This is that the little daughter, whom I shall call Lettie, and who was just one year and nine months old, died on December 2, 1897, from the burning of its cradle.

At odd times between August and December Mrs. D., in her thoughts about the child's future and while planning some little thing for her, would hear a voice saying, "She'll never need it." One of these occasions was the following: The family live in a house with few accommodations for a clergyman who requires a study, and Mrs. D. planned to give Lettie a certain room for a bedroom when she grew older, and was running over how she would furnish it, and this voice came as described. It was not exactly what one could describe as an external voice,

nor again a mere thought impression or product of the memory and imagination, as we usually characterize such things, but one of those internal voices with which psychical researchers have become familiar and which Mrs. D. herself distinguishes as neither a real voice nor a memory reproduction, but an impression with all the characters of a real voice except the sense of external reality. Psychiatrists will recognize without remark the nature of such an experience, and as I am only narrating facts I do not require to make any comments.

There were many repetitions of this voice in about the same language. One of them occurred about two weeks before the child's death. Mrs. D. had resolved to write a little diary which she could give to the child when it became older. She wrote down two separate accounts on different days of certain events having an interest to the little girl, the day of the month, unfortunately for the psychical researcher, not being mentioned in them, though this would have been of no importance for the contents of the diary, as there is nothing evidential in them regarding the incidents at hand. But while writing them, this voice came as before: "She'll never need them." The day before the child died the same voice appeared, and on the morning of its death the child was running about the house in a rather dilapidated pair of shoes, when Mrs. D. remarked to the child that her feet must be cold and thought she must have a new pair of shoes. In the midst of her thoughts came the voice again, "She'll never need them." It must be added also that, previous to the impression of a coming 'burden' above described, this voice had been heard several times.

About a week before the child's death Mrs. D. thought she smelled fire at night, and feeling afraid of it went to the cellar to look after the matches and to see that there was no danger. She found no traces of fire and nothing to explain her impression. But from that time she began to be careful about matches, seeing that they were in safe places and out of reach. She even went so far as to look over the house for the matches, and felt a strong impulse to burn all parlor matches which were of that kind that is easily lighted. Once the impulse to do this was attended with something like a voice warning her to the

same end, and about the danger of fire. Nothing definite enough having been suggested by the voice to guide her actions directly, Mrs. D. could only imagine the necessary precautions, and finally thought to hang a dripping pan in front of the range fire, a thing Mrs. D. had never done before, to prevent coals from falling out during the night. Nor had any apprehensions of this kind ever been felt before, within her recollection, and there were no special reasons to suppose that any danger of fire in this way existed. But as there was no other fire in the house than that in this range and one in the heater, a sort of closed stove or furnace like the Baltimore heater, no other definite course was left open to the imagination for preventive measures except the unusual one mentioned.

On the morning of the child's death, and during family worship, another incident of some interest occurred. In the midst of the petition for individual members of the family, when she came to the phrase with which she besought divine care for each one, and attempted to apply it in behalf of Lettie, though no difficulty was encountered in the case of the other children, in this case something seemed to stop Mrs. D.'s voice, and she could not repeat the usual language. She recalls no similar previous experience.

On the same morning, about an hour before the fatal disaster, the propulsion to destroy the matches that were dangerous became stronger and stronger, until Mrs. D. turned and reached for the box to destroy it. But as she picked it up she thought, No; L. (the elder boy) is gone, and she thought that she might need the matches to light the gas stove. She then said aloud to herself, "I'll destroy it as soon as he comes back." She then went on with her work in the kitchen. When the time came, about ten o'clock, Lettie was taken up to her crib for the morning sleep, and as Mrs. D. was putting her into the cradle a voice, such as has been described above, said: "Turn the mattress." This Mrs. D. was accustomed to do, though she had never experienced any voice before in connection with it. But, being in a great hurry, she simply said in a motherly way to the child that she would turn the mattress after the child had taken her nap. She then went down stairs to her work. After

a while she heard the child cry, and hurrying up to the room, found the crib and its bedding on fire, and the child so badly burned that it died in three hours.

The only possible way to account for the accident was to suppose that the child had found a match, possibly in the crib or on the mantel piece, which she could reach, and lighting it, had set its bedclothes on fire. The other two children were not present. L. had gone down town on an errand and E., the younger boy, was at school. No fire was on this floor of the house, but in the kitchen and the dining room, both below.

Now, another incident of much interest had occurred many times during the two or three years' residence of the family in this house. Mrs. D. had often had a visual apparition of this very crib on fire, but, as her apparitions or visual automatisms are very frequent, she had not thought to assign it any meaning or possible coincidental value until after the accident.

These were the experiences of Mrs. D. previous to the event, but there were two other incidents by other persons than Mrs. D., that lend themselves to a construction of coincidence in connection with the accident. The first is exactly like the one narrated as occurring at family devotions. Mrs. D. has a sister living in Connecticut, some seventy-five miles from B., the home of Mrs. D. No correspondence has passed recently between them, and the sister was not given to as devotional a life as Mrs. D. It must also be remembered that the sister had ridiculed Mrs. D.'s stories of her experiences, and even went so far as to half jestingly criticise Mrs. D. for her extravagant piety. She discouraged Mrs. D.'s tolerance of possible significance in many of the coincidences which I have recorded in the Proceedings of the Society for Psychical Research (Vol. 12, p. 259 seq.), when they were the subject of conversation. But on hearing of the child's death she came to B. and narrated an experience of her own. It was to the effect that about a week before the death of the child she had had such an experience as she had never had before. An overwhelming impression of some great calamity to occur in the 'family' (the incidents show that the term included the whole family connections), and the impulse arose in her to pray for each one, which she did, feel-

ing, as she expressed it in her narrative, that this was an unusual procedure for her. She went over each person among parents and relatives, until she came to the child, Lettie, when her voice suddenly stopped and she could not pray for her as for the others. She finally managed, however, to utter with struggling voice a petition for 'our little blossom,' the name which she was accustomed to apply to Lettie when speaking of her.

The second incident was an experience of the next door neighbor to the D.'s. I shall call the lady who had it Mrs. G. On the afternoon of the child's death Mrs. G. came in about three o'clock and apropos of the accident remarked that on the night before, I believe it was, she had been wakened by the fear of fire and had gone down to the cellar to search for it, and exclaimed while making the search: "Oh! if our little baby would burn up!" Her own child was about the age of Lettie. The relation of this incident to the case will be noticed later.

There was also another experience of Mrs. D.'s which psychical researchers would classify as 'symbolical.' Whether it be so or not is a matter of no concern to us at present, but is recorded for the sake of the interpretation which the mind is capable of putting on it either as an afterthought or as a confirmatory coincidence of the others. But a night or two before the accident Mrs. D. had a dream with the following incidents in it. She had gone with Mr. D. and the three children to the railway station at M. to take the train for a visit to a friend. As they came up to the station the train was coming in. Mr. D. with the oldest child, L., ran across the track ahead of the train and reached the platform. Mrs. D. and the other two children were too late to get across the track and waited until it stopped. They then climbed upon the car platform to cross over and join Mr. D. and L. But, just as they reached the platform, the train began to back upon a switch, which was the custom at this place to let a train pass. Mrs. D. paid no attention to it, but started through the car expecting to find her husband and other child. She noticed that the train was empty, but, leaving the two children with her in a seat, went on in

the search for Mr. D. and L. Presently she found that the train kept backing and backing until she noticed that it was near Toledo, some forty miles from her starting point, when she came upon the conductor, who told her that the children, E. and Lettie, had been switched off some time before. Mr. D. and L. reached their destination safely and were joined later by Mrs. D. Such was the dream. Now, since the death of Lettie and during the funeral Mrs. D. has frequently heard a voice say, "The end is not yet." Mrs. D. also narrates that she often has a feeling that E., the child here associated with Lettie in the dream, may get killed by the trolley cars, accidents of this kind being frequent in the city where the family lives.

These experiences took place before the death of the child. There are two others, however, that occurred after it and that may throw some light upon all the phenomena purporting to suggest coincidence.

The night after the burial of the child Mrs. D., as perhaps is true of most persons passing through a shock of this kind, could picture to herself nothing but the little coffin and the grave. It was not a vision or an hallucination, but only a memory picture, such as any one can recall. The remembered picture was exceedingly unpleasant, and, evidently, in spite of her faith, a little tinge of scepticism came to disturb her mind, because she said that she did not like to think that her little child was not a spirit, but a corpse with a vanished soul. To remove the unpleasant feeling Mrs. D. prayed to have a realizing sense and the power to know that the child was a spirit and did not lie in the grave. At this time she was at the home of her sister, whither the family had gone to seek a burial place. One morning, soon after this prayer, she awakened and lay for an hour thinking over family affairs. The sun was shining brightly in the room, and while thinking about the clothes she would put on the two boys to prevent their best ones from being soiled at their play when they got up, suddenly she saw a form by the bedside, and, turning, saw an apparition of little Lettie with her hands on the bedside and smiling at Mrs. D. By her side was the form of a woman, holding her hands about the child, as if to assist it. Mrs. D. sprang up in bed and uncon-

sciously exclaimed, "Good morning, Lettie," and both figures immediately vanished. The forms were transparent and objects could be seen through them. The grown form was not recognizable as any one that Mrs. D. knew, but it had no distinct resemblance to the representation of an angel, such as pictures might suggest. It seemed, therefore, not to be an automatism from the memory of angelic pictures. The dress fitted rather closely, and the hair was of a decidedly golden hue and the face one of great beauty. No suggestion of friends was apparent in it. The experience displaced the ugly feeling created by the memory of the coffin and the grave, and though not believing that she had seen the spirit of her child, or that spiritualism is a rational doctrine, Mrs. D. retained a strong sense of satisfaction from the vision. She is disposed to interpret it as a providential comfort for her sorrow.

At the end of December another incident took place that will have some interest. This time it was the experience of the little boy E. It was first told me by Mr. D., who had called on a business matter. It seems that the child had climbed up on a couch beside his mother who lay down for a rest, and in a few moments asked his mother if his sister Lettie was smoke. The following letter from Mrs. D. in response to my inquiry narrates the details of the occurrence.

B——, January 5th, 1898.

DR. HYSLOP:

You requested a note of E's recent experience. It occurred on Thursday eve, Dec. 30th (1897).

I lay down on the sofa to rest in the evening, and, as he often does, he climbed back of me to rest with me. I do not remember what my thoughts were, but feel quite confident I was not thinking of my experience at S——, Conn., when E. said: "Mamma, is little Lettie air now? Is she like smoke?" Why, darling? "'Cause I just saw her and put my arms around her and she was like air." I will endeavor to keep account of anything further.

Yours respectfully,

E—— D——.

On inquiry about the incident I could find no trace of any story to the child that might lead to a belief on its part in such a reality as its experience might be taken to describe. Neither Mr. nor Mrs. D. could recall any narrative that might suggest it. No immediate thought or statement of Mrs. D., who was

intent on rest, could be recalled that might have inspired the child's idea. Moreover, the child was only four years old. The incident impressed both parents as very striking, and they were evidently puzzled by it, having a strong aversion to the apparent meaning of such occurrences.

Such are the facts, or at least alleged facts, in a case of real or apparent coincidences. I must warn the reader, however, that I have not narrated them either for the purpose of proving any hypothesis or with the demand that any one shall consider them genuine or significant. I am content if I have produced an average story of this kind which can at least pretend to authenticity. I am willing to concede any amount of scepticism in regard to the importance of the alleged experiences, since it is not a part of my task either to vindicate their authenticity beyond question or to urge their extraordinary interest. Any man may have what theory he pleases about these matters. The plan here is to produce facts in the same individual experience which science will have either to question equally with the above in order to save its consistency or to accept the whole with their defense of psychological interest for even suspicious phenomena. Nevertheless, since it is the intention to show more fruitful sources of difficulty to the supernormal than illusions of memory, it will be necessary to recognize the question of authenticity and allied problems. But the main purpose is to study the individual case and to find in it the explanation of what one side may regard as supernormal and what the other ignores simply for the lack of courage to study the facts.

I think that every one would frankly admit that the narrative presents, at least to the ordinary mind, an extraordinary set of coincidences in favor of premonition and spiritism. That is the interpretation which the temperament of many persons would put upon the incidents, and their apparent relevance for this purpose is all that I care to sustain. The impression that such experiences make on the average man or woman is all that it is necessary to recognize in order to demand for them the same consideration which mesmerism and reports about meteors were finally able to exact, much to the shame of those

who at first insisted upon laughing at them. For myself I do not wonder that untrained psychologists feel greatly puzzled at such incidents, when I come to consider the marvelous fertility and complexity of mental processes. After all, science is founded on coincidences of some kind, and it cannot afford to dismiss them hastily, when a little tolerance and patience will reveal a rich field of explanation, without discrediting facts on the one hand, or rushing into the arms of the supernatural on the other. In the present case some of the facts certainly simulate the view of premonition and others equally simulate a spiritistic interpretation. I think few persons would question this assertion. But refusing to treat them conscientiously will neither dispel the illusions so freely imputed to others nor discover the causes of their apparent significance.

The first criticism which I imagine the average psychologist would direct against the supposed value of the alleged coincidences narrated would be the vague indefiniteness of the feelings spoken of as premonitory. This I have mentioned for the sake of conceding it as fatal if the question concerned their evidential character in behalf of the supernormal. But, inasmuch as I am less anxious to either prove or disprove the extraordinary nature of the phenomena than I am to discover in this individual case the possible influence of other agencies quite independent of both vagueness and distinctness, I may assume that the case is free from that objection. Besides the accusation of indefiniteness cannot so easily be brought against the incidents of the apparition of the burning cradle and the automatism or voice "*She'll never need them.*" Nor is there any vagueness about the apparition of the child after death. But I shall grant, for the sake of the argument, that the facts are too inconsequential to seduce severe scientific method from its attitude of skepticism, in so far as the supernormal is concerned. It will not be so easy, however, to *explain* the coincidences as it will be to doubt their evidential value for occult theories. But, as the more definite experiences yield to easy normal explanation, when the mental habits of the subject are known, we may easily dispose of the less definite incidents.

The spiritistic interpretation, I have said, is a natural one

for these incidents. But the difficulties with which that hypothesis has to contend are much greater than the narrative would suggest, and they can be discovered only by a direct investigation of the mind that had the experiences. To make this evident, I have to remark many more facts than are even likely to spontaneously find their way into such a story. They are all included, however, under the general head of automatisms. This term I use to denote any resurgence into consciousness of either an apparent reality or an idea wholly foreign to the contents of the present stream of thought and in no way impressible into it. They may be called by any other name that is desirable. If the reader prefers the term hallucination I shall not object. But I choose 'automatism' as less invidious in its implications. I have found these experiences very frequent with Mrs. D. Many of them have been closely connected with her religious life, the automatism taking a form that associated it at once with an intense devotional piety. For instance, the habit of devotion in moods of religious want was intimately associated with promptings to pray at the most unlikely times and in the most unlikely places. Religious reflection seems to have instigated certain tendencies to a strong and persistent emotional life that had an associative influence upon the stream independent of the ideas immediately in the field of attention. The consequence was a large number of automatisms, often capricious, but traceable to the subliminal trend of her emotional life. Pierre Janet's conception of the 'disintegration of personality' affords a good representation of what went on in her mind, though not at all so marked as in his cases. Her religious emotions were either persistent with all the incidents of everyday life or were subliminally active when they had no natural connection with the main stream of mental action. As an illustration of this I may mention an instance of crystal vision which I have already put on record from the same subject. ('Proceedings of Society for Psychical Research,' Vol. 12, p. 261.) This experience represented a vision of a room with sunbeams pouring into it through a recessional window, and into the stream of sunlight flew a dove. Remembering that many religious books and pictures have associated sunlight and the dove, I inquired to know

whether Mrs. D. was familiar with such representations and found that she was, just such a picture being on one of the family Bibles. She herself did not recall any such until her husband first responded to my question in the affirmative, showing that the association was wholly subliminal. Another beautiful instance of purely subliminal association will be found in the case of Miss X. ('Proceedings of Society for Psychical Research,' Vol. 8, p. 484.) It quite resembles the one by Mrs. D. Now the room into which Mrs. D. saw the sunbeams pouring exactly resembles a corner in a mediæval church. This would naturally appeal to the religious emotions and associations. The tendency of the mind under such conditions requires no further comment for the psychologist. Nor is it necessary for the subject to be able to detect the trend of consciousness in the case. The influences are too subtle to be traced easily. But they are there, and have to be reckoned with in the explanation of all data not properly fusible with main stream. Another beautiful series of automatisms with Mrs. D. are connected with the playing of the piano. She has had no special training for this, and has picked up mostly what little she knows by herself. About two years ago, and all at once, without any practice, a piano having been provided only a short time before and no regular playing having been indulged for a long time, Mrs. D. noticed herself playing pieces automatically and sent for me to know what it was. She was wholly unconscious of intending the movements of the fingers or the pieces of music played. Some of the pieces played were wholly unknown to her. Some were familiar hymns and some were a combination of various familiar pieces of church music. In listening to the instances of unknown pieces I noticed that they were of the same type as the familiar hymns : They were all of the religious class. I found on inquiry that music has a strong influence on her religious emotions. This effect would react on the piano playing, so that any emotional phase of her mental life in the field of attention or out of it, that is, supraliminal or subliminal, might either recall the past into consciousness or automatically reproduce music that she might or might not recognize. That she might not recognize some pieces is easily rendered probable

by the experience which she narrated to me about the sky, garden fence, chain pump, etc. (see 'Proceedings of Society for Psychical Research,' Vol. 12, p. 262, 263), and which illustrates very clearly both the fact of automatism and recall without recognition. In the musical instances we find the influence of the main trend of religious emotion. Not that this is the only emotion that is likely to produce them, but that there is a unity between this fact in her life and the musical automatisms observed. That circumstance suffices to establish a principle to be used here as a basis of explanation.

We have now a fulcrum to apply in the case of the incidents connected with the present narrative. Take first the apparition a few hours after the funeral. This is one of the decidedly spiritistic incidents of the case. But if the reader will return to it, he will find that the state of mind that preceded it was precisely one that might lead to just such an automatism as the experience records, if it be an automatism. I hardly know a better fact to suggest automatism originated by latent influences in the system than this very incident. There was confessedly a strong wish to remove what is in reality, though not perceived as such, a skeptical feeling about spiritual survival from the grave. There was a desire and a struggle to get rid of an unpleasant fear, impression or memory, and the act of prayer would tend to restore the old faith and its influence upon the mind. The physical exhilaration of the sunlight and fresh morning air in the country might produce a favorable condition, subliminal or supraliminal, for the resurgence into consciousness of a suitable object of consolation. What more likely then than that the mind should succeed in pushing forward some experience which would take the place of the unpleasant sensation that had instigated the prayer? Having had many experiences of visions, aural automatisms and impressions, evidently determined by those conditions of mind not immediately occupied with the object of apperception and closely associated with religious wants and emotions, we can here trace a possible influence from the latent expectation of consolation to relieve the disagreeable feeling connected with a half skeptical tone of mind wholly foreign to her regular life.

The psychical researcher may think this explanation rather far-fetched. This may be true, and I do not care to urge it as determinately true beyond all doubt. I am satisfied if it can appear as an alternative possibility to the spiritistic theory, for that fact will put limitations upon the theory that claims at least superficial recognition.

Another interesting incident in the narrative points in the same direction. This is the case of the apparition of the burning cradle. It is one of the most striking coincidental features of the whole narrative. I have mentioned the experience without any of the circumstances that personal inquiry produced, in order to keep the incident in the shape that such facts usually take where the antecedent circumstances are not investigated. But now if we inquire into these we shall find a possible explanation, certainly preferable to anything like premonition until that hypothesis obtains satisfactory credentials elsewhere. The fact is that the crib stood within a few feet of a fire grate. But as there had been no fire in this grate for a year or more the accident could not have been caused by this, a circumstance mentioned to sustain the theory above advanced to account for the accident. In the first place, Mrs. D. herself had all along explained the vision of the burning crib by this very proximity to the fire grate. In the second place, almost every one would have such a possibility suggested to the mind by this situation of the crib. But not every one is subject to automatisms, and such thoughts are easily referred to their proper source in association. Mrs. D., however, as we have found, is liable to these occurrences. Besides the associations of others, whether supraliminal or subliminal, these influences are liable to provoke an automatism independently of the main stream of consciousness. Now it is the unusual occurrence and character of automatisms that call special attention to them. They are easily remembered as interesting and significant if any coincidence with them is remarked. If the accident of the child's death had occurred only in connection with an *association* of a burning crib, every one would have dismissed it as a coincidence not worth taking seriously, and no significance would be given it. But when an accident of this sort occurs in coincidence with an *apparition*

apparently premonitory in character, we forget association and are tempted by the unusual nature of the phenomenon to ascribe it a value that it may not deserve. We may concede that such an experience might have some significance if not connected with automatisms as frequent or habitual occurrences. But here we have in this very subject the existence of automatisms which can be traced directly to emotional influences of various sorts. There is a frequent connection between past thoughts and associations and certain sensory automatisms, and we have only to suppose this case one of them in order to explain it in a natural way. Mere association in this case would not have suggested significance. Hence, as there is a probable connection between a frequent association and an interesting coincidental automatism, there will be no more reason to give the latter a significance than the former, which is never inclined to receive such importance. If the content of the automatism and its complications are independent both of habit and hallucinatory suggestion, there might be an excuse for suspecting an importance for it. But there is no more apriori reason for giving evidential value to an automatism than to a suggestion from association. Consequently, when we find an experience in all probability only a more developed product of association, which does not obtain any transcendental significance, a product in which central activities effect the work of peripheral stimuli, the central action being nothing more than association or suggestion, as in dreams and hypnotic hallucinations, we must not be in haste to attribute to it other than the normal psychological value, although it has other than the normal psychological cause.

Another incident is amenable to the same explanation. It is the case of the automatism occurring at the time the child was put to bed. The aural automatism, "Turn the mattress," can easily be accounted for by supposing that the natural resistance of memory and association to the resolution not to turn the mattress at the time might readily produce the result. Of course, we should not expect any such occurrence in the average person, but we have here a case constantly exposed to it, and also the two known facts that she was accustomed to do the very thing indicated by the voice and that this very thought was

consciously urgent on the mind until the resolution not to turn the mattress was formed. The automatism, "Turn the mattress," was then probably nothing more than an hallucinatory resurgence of the thought that preceded that resolution, the impact of habit and association against the new course adopted.

The two incidents just considered were of the premonitory type, and could be brought under one general explanation. The next is not so easy to explain in the same way. But it may be made to yield to a more complicated analysis. The incident is the little brother's apparition of his sister a month after her death. This is certainly very interesting, whatever we may think of its value as evidence for transcendental existence. It has a more decidedly spiritistic appearance than the other incidents. Nevertheless, its cogency is subject to limitations which, though complicated, ought to be carefully considered.

The first objection to its evidential character for the spiritistic theory is the doubt about the source of the child's idea of his sister after her death. But as my object is not to risk the case on the impeachment of testimony, I wish to deal with the case as if it were not subject to skepticism at all. Assuming then that the apparition was in no way a direct suggestion of the parents, I have to look for an explanation outside of spiritism. Now, on inquiry I have found incidents that may lead the way out of this supposition. I find that Mrs. D. has noticed a great many times since the occurrence that, while she happened to be thinking of the child or even other matters, E. would speak up and mention her or the subject of his mother's thoughts. This has occurred so often and in such circumstances of a peculiar and unexpected sort that Mrs. D. herself remarked the possibility of accounting for the original phenomenon by telepathy. Unfortunately, however, she kept no record of these observations, the contents or circumstances of the alleged coincidences, and hence there is nothing to go upon except her own unsupported judgment in regard to the cases thus mentioned by her, and they can pass for little worth. But an interesting incident occurred somewhat later which is more important in favor of a telepathic explanation, though this hypothesis depends upon prior proof of its truth for its application here. The incident

suggesting this view of the occurrence I obtained the next day after its occurrence and without its bearing being anticipated by Mrs. D.

Mrs. D. had retired early, and, awakening early, had got up to go to the kitchen about 3 o'clock. After she had reached the kitchen, and without any reason from previous habits or thoughts or from any known circumstances about the house, she suddenly felt a fear come over her that there might be a burglar in the house. She thought at once that such a feeling was nonsense, but it clung to her, and she looked at the window to see that it was secure, and turned to come back to the bedroom, when she saw a door open several inches and by which a man could easily have entered. Just as she started to close it, E., whom she had left sleeping in the next room and in no position to know anything about the door, awakened and called to her. Mrs. D. simply went on to close the door before responding to his call, and he again called out, impatiently, "Mamma, I was dreaming that burglars were in the house." Now, if we treat this coincidence seriously at all, dismissing the possibility that the dream was a suggestion from her own movements in the room, at least for the sake of considering the other view, we might suppose it due to telepathy. If then we were to tolerate this hypothesis to explain the coincidence in this instance, we may extend it to the case of the apparition, remembering that advocates of it do not maintain the necessity of present active thoughts to the result, inasmuch as the process may be wholly subliminal, as well as supraliminal. It will be remembered that Mrs. D. was not thinking of the deceased child when the little boy's apparition of his sister took place. But at any rate, if we consider telepathy in the case that has no suggestion of spiritism in it, the same hypothesis should be applied to the other coincidence if it permits of the application, as I think it does.

But there is another resort that may commend itself more favorably to the average scientific mind, if he does not admit the existence of telepathy. This supposition is effective against the spiritistic theory to those who accept telepathy, and hence I have the advantage of using it for that purpose where there is any disposition to treat the coincidences seriously at all. But

dismissing the coincidence about the burglars either as a chance case or as a suggested dream, I was able at the time that the experience of the apparition was told me to ascertain some interesting facts that suggest a possible explanation for it independently of both spiritism and telepathy. A careful inquiry into a number of facts which I shall not take the space to describe in detail, but which were very suggestive, led me to believe that both the remaining children have some hereditary susceptibilities like the mother. Assuming this as probable at all events, and remembering that inquiry into the habit of the child E. showed he had been accustomed to lie on this very sofa with his little sister before her death with her in his arms, just as he described her in the apparition, we have only to suppose that suggestion might give rise to the apparition itself. This will, no doubt, appear a complicated and round-about explanation, but with the indications of hereditary peculiarities in the child and the wide range of automatism in Mrs. D. we may well halt before going farther for an explanation.

There is also an interesting feature about Mrs. D.'s impression in regard to a burglar in the house. She knew no reason for its occurrence, as she had not been troubled with such feelings before. This also yielded to inquiry. I asked her whether in going to the kitchen she had to pass near enough to the open door to have a current of air come in contact with her, and the answer was decidedly in the affirmative. It was dark and she did not see the open door, nor did she consciously feel any draught of air, but the door opened into a hall from which a current of air could easily come, and this is a common fact in apartments of the kind in which the family live. Assuming such a draught of air, and with it, first, Mrs. D.'s liability to automatisms and, second, the possibility of subliminal reasoning, such as Professor Newbold reported ('Proceedings of Society for Psychical Research,' Vol. 12, pp. 11-21), we get an explanation of the automatism itself without resorting to the supernormal, even if we disregard the possibility that it was a chance suggestion of what may be and is a common thought in the large cities at that time of night.

We have now disposed of some of the most striking incidents

of the report, and there remains one very definite case not so easily explained away; namely, the automatism of the voice, "She'll never need them," and the precautions about matches. The consideration of the latter incident opens up another aspect of the problem. It loses its significance at once when we ascertain, as I did, that Mrs. D. all her life has been very careful about matches and has often reproached her husband for carelessness in this matter. But there is an aspect in the incident that brings up the problem of memory, and the method which I have to criticise in the work of Parish.

From what I have remarked about Mrs. D.'s lifelong habit of care in regard to matches we can easily see that the coincidence between her similar action just previous to the child's death and that event itself, or rather the supposed significance of it, is an *afterthought, due to the very strength of the subject's memory rather than its weakness*. We may say that there is dissociation of the habit previous to the time of the other automatisms, and thus recognize a measure of defence for the contention of Parish, but with the ordinary memory not commanding so many of the smaller details of life the connection between the caution about matches and the accident to the child would hardly occur. It was probably the very keenness of Mrs. D.'s memory that enabled her to recall the circumstance which creates the coincidence. The illusion I cannot regard as one of memory, but rather as one of apperception or judgment, which is likely to occur with persons not accustomed to scientific observation. Had the subject been antecedently aware that events and experiences preceding those constituting the coincidences recorded determined their value for or against any hypothesis, it is probable that the apperception would have been different. But in the absence of any knowledge of such necessary precautions the common mind very naturally seizes upon the events contiguous in time and apparently relevant to the one which they seem to portend, and the defect, even if complicated with some dissociation, is mainly one of apperception occasioned by the very keenness of the memory for the incidents of the past which can be made to appear significant. Moreover, it is probable that the more striking coincidental automatisms and

the memory of them had much influence upon the recall of the feelings about fire and apperceptively distorted their significance, so that much more than illusions of memory, in fact, phenomena much more important than they, have to be reckoned with in the treatment of such reports as the coincidences in my narrative represent.

To reinforce the view that the defects in such a narrative are likely to be something else than illusions of memory, I was careful to keep a watch for such errors. I have watched for them during the several years of my observations in this particular case, and have not been able to discover a single one. I have found cases of obliviscence, and they have been quite interesting as enabling me to discover, by cross-questioning the subject, that the source of some of the automatisms was an associative resurgence into consciousness of a past experience, taking the form of an hallucination without recognition. But when recognition was made I have found no reason to believe that an illusion of memory had occurred. But at the same time this pious opinion of mine can go for very little value to the outside reader. Hence I have presented nothing here which I did not seek to corroborate by another witness, which in this case is Mr. D. I had also an indirect opportunity to confirm this conclusion. The story by Mrs. G., a neighbor of the D.'s, was the occasion of it. I ascertained the facts of Mrs. G.'s experiences, as above narrated, from her own statements, and found that they were exactly as told me by Mrs. D. This instance afforded me a good chance to test Mrs. D.'s value as a witness and the confirmation of my impression about her in this respect, and serves, at least negatively, as an injunction to look far more deeply into such narratives of striking experiences than the possibility of mnemonic illusions suggests.

In the investigation of the neighbor's, Mrs. G.'s, experience which was told as if it might be taken as premonitory, I came across an interesting fact that confirms the whole position here taken. I inquired, as usual, to know whether similar fears about fire had been common, and besides a number of instances of such fear, I was told of one which Mrs. G. described as quite a remarkable 'presentiment.' To make a long story

short, she described the discovery of smoke in the hall and the suspicion of danger from fire, and after warning her neighbor of her fear she had finally to call in an officer of the law to interfere, and found that her conjecture was correct. Here is a case where the only difference between the psychologist and the common mind is the choice of language. 'Presentiment' is the term chosen to express an *inference*, a fact that reveals the frequent need for investigation into the mental habits of the individual in order to discover the real explanation of phenomena that often appear remarkable. This conclusion, however, is not a new one, but perhaps very trite. Nevertheless, when psychical research presents such an enormous mass of facts as its reports represent, it is incumbent on the critic to subject some equally good cases to the analysis of a personal investigation, and not to rely exclusively upon general principles inductively obtained from incidents that make his argument seem *a priori*. The criticism should be based upon strictly analogous incidents.

The automatism, "The end is not yet," gets its interpretation from the apparently symbolic dream in which E. was included with the sister, who died, as separated from the mother. That it should have any meaning at all is an afterthought or apperception created by the coincidental character of instances more suggestive than it and taken with the remembered fact that Mrs. D. has often felt that E. might be taken by the trolley cars. There is an aspect to this incident that makes it like those which I have explained by suggested automatism, though it should be remarked that the narrative does not make it anything more than an association which is very common in the city where the family lives. The only feature about the impression that seems to give it possible meaning is the fact that the same feeling of fear did not and does not occur in reference to the older boy L. Hence taking the automatism, "The end is not yet," and the symbolic dream with this impression it might be natural for the untrained psychologist, especially in connection with a large number of coincidental experiences not mentioned in the present narrative, to wonder whether the circumstance might not have an extraordinary interest. On inquiry again, however, I found that Mrs. D. had more confidence in

the ability of L. to take care of himself than in E. The older, L., is more independent and self-reliant than E. and has thus been better able to take care of himself. E. has always shown a disposition to depend on his mother, and she a solicitude for him that she has not felt for L. Now, if we put together the anxiety which every mother in this particular city feels for her children who are exposed to the dangers of the trolley cars, Mrs. D.'s special concern for E., and more particularly her liability to automatisms, we have mental conditions that strongly favor the occurrence of impressions which might be taken for warnings of a premonitory kind with those who have felt the touch of sorrow in connection with such a collection of coincidences as I have here recorded. It must be remembered, however, that Mrs. D. has never believed in premonitions or presentiments, so that the discovery of the coincidences was not wholly a product of apperception due to a tendency to seek for them in the afterthoughts. On the contrary, for the first time in her life, she and Mr. D. were amazed at the extraordinary character of the incidents in this narrative, in spite of some coincidents of another kind and interest which I have put on record elsewhere, and came to me to ascertain whether I had any ordinary explanation for them. Afterthought and apperception being shut out as inadequate to the result, even after allowance is made for their participation in it, we find, I think, evidence of an extraordinary combination of emotional interests and a predisposition to automatisms to simulate supernormal phenomena.

Reference to the narrative, which shows such a cumulative mass of incidents at least apparently in favor of premonition and spiritistic theories, will show that I have suggested a normal explanation for the majority of them, and only a few have been omitted from review. The incident of the sister of Mrs. D. hardly requires notice, as I have not been able to apply the method of studying her mental habits, and it may be too vague to deserve consideration. It was mentioned because it at least simulated the *collective* character of incidents in the psychical research records, and in order to give the case all the superficial cogency of which it was capable. But it must run the

gauntlet of the method applied to the other incidents before any interest of an extraordinary kind is attached to it. On the other hand, I confess that I have not found any satisfactory explanation of the repeated automatism, "She'll never need them." But if I have broken the cumulative force of the whole by presenting a possible explanation of the majority of the most strikingly spiritistic cases, we may well suspend judgment upon this one unexplained incident. The main point has been gained if I have shown that no extraordinary amount of illusion and hallucination is required to explain such phenomena, but that they may be made to yield to a critical analysis of the individual experience and the usual processes of mind. Consequently, while we may both admit and urge the importance of the position taken by Parish, we may reserve to skepticism and scientific method a resource much more far-reaching and effective than his, and, when his either breaks down or proves too much by casting doubts upon the accepted authenticity, methods and results of previous science. If the liability to mnemonic illusion and hallucination be half so great in such phenomena as Parish criticises, and they certainly are great, we should have to revise the results of previous psychology more than the critics of psychical research are inclined to do. Moreover, I am disposed to think that mnemonic illusion is much less frequent in extraordinary experiences than in the ordinary, while Parish proceeds upon the assumption that it is more likely in the former. But we must remember that illusion and hallucination are a two-edged sword and cut both ways. They will discredit the claims of the ordinary at least as much as the extraordinary, and I think more. Hence, while admitting their extreme importance in all judgments of experience, I am inclined to believe that a far profounder source of difficulty to psychical research can be obtained in the field which I have here endeavored to explore, and certainly one left open after the other fails. It is a resource, also, that can be employed only by abandoning all arrogant pretensions to *a priori* knowledge about such phenomena, and by condescending to study the individual case at first hand.

VISUAL PERCEPTION OF THE THIRD DIMENSION.

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Since the time of Berkeley the most important particular problem in the general discussion of space-perception has been the explanation of the visual perception of depth. The objective conditions on which such perception of depth depends may be isolated more easily than the conditions for any other form of space-perception. As a result, the analysis of the subjective phenomena is here very much easier than in other cases. The conclusions reached in regard to this particular form of perception are, however, applicable to the more general problem ; for the visual perception of depth must be regarded as similar in kind to all other forms of space-perception. When the particular case is taken up for discussion, then, as it is to be in the present paper, the aim must be to reach general conclusions which shall bring the whole problem nearer to a final solution. And it will be possible in the course of such a discussion to test in this particular sphere some of the general theories that have been advanced.

First of all, then, let us raise the question, Does the presence of a third dimension in ordinary binocular and monocular fields of vision justify the doctrine that the sensation factors, which are the elements of the completed percepts, have an original attribute of extension, analogous to quality and intensity but differing from these? This doctrine has been advanced by a number of recent writers, and it must be granted that it seems a simple and direct hypothesis. It is true that we never observe a sensation except as part of a percept, and every visual percept has its projection into the third dimension. It follows from this that we can never observe visual sensations apart from some spacial relation. But while granting this, it is not necessary to conclude further that these spacial attributes are attributes of the sensa-

tion factors themselves rather than of the percepts in which they appear. Empirical evidence of an indirect character can be found which will aid in supporting this position.

If we take a thread of several meters in length and let it extend from the bridge of the nose to a distant point which is somewhat above or below the plane of the horizontal position of the optical axes, we shall observe double images, whatever point on the thread we fixate. If we fixate any point other than the extreme end of the thread we shall see a figure *X*. This *X* is due to the fact that the double images fuse at the two points of clear vision and do not fuse at other points. Now fixate some point along the thread such that the nearer and further halves of the *X* shall appear to be equal. The distance will vary according to the angle of elevation or depression of the thread, but it will always be found that the nearer half of the *X* is in reality very much shorter than the further half. I find, for example, that with a thread 330 cm. long and sloping at an angle of about 30° upward, the point of fixation is only 45 cm. from the bridge of the nose. Figure 1 represents the thread (*AB*) and the image (*ab*) in one eye.

If now the point of fixation coincided with the real middle point (*G*) of the thread, it would follow that one part of the image (*ag*), while representing no more depth than the other part of the image (*gb*), would have greater extension on the retina. If the thread were fully recognized as a three-dimensional object, this relation of the parts of the image would also be recognized. As a matter of fact, under ordinary circumstances, the thread is regarded as projecting somewhat into depth. If, however, the associated factors that come from a familiar field filled with objects are removed, this projection into depth becomes less and less clear. In no case is the true relation between the parts of the image fully recognized. This failure to recognize the meanings of the two parts of the retinal image goes to show that the third dimension is, under the circumstances described, very incompletely recognized. This can

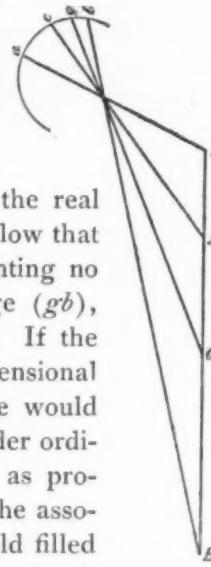


Fig. 1

be still further emphasized by noticing the real principle of division. If the retinal image, instead of the thread, is divided into two equal halves by the center of clearest vision, the point of fixation on the thread (*f*) will lie very much nearer *A* than it does to *B*. This is what actually takes place in the experiment described. If *AB* is taken as 330 cm. in length, *Af* will be 45 cm. But the principle that one-half the retinal image equals the other half holds only for two-dimensional interpretation. We have here then a series of facts which indicate that three-dimensional attributes are secondary and derived forms of perception rather than original attributes of sensation. The experiment is not direct or complete, but it shows that, if some of the factors of an ordinary percept are removed, so that we approach more nearly to the original sensation elements, we find the spacial attributes much less complete. The earlier interpretation is clearly two-dimensional, not three-dimensional.

Further evidence which goes to show that all the objects in the monocular field lie in a single plane, that is, are not distinguished as having different degrees of depth, may be found in an earlier article devoted to that special discussion.¹

In none of the cases brought forward has it been possible to show that sensation factors do not have some three-dimensional attributes, but it has been shown that the complete perception of the third dimension is the product of complex rather than simple conditions. The simpler the conditions, the less complete the three-dimensional attributes. It must be insisted that this incompleteness of the third dimension is not merely a matter of measurement and subdivision of a perceived dimension. But while in some cases it expresses itself in the form of a judgment which does not correspond to objective measurement, it is always an incomplete perception. The measurement is not uncertain or vague to the mind of the observer, it is incorrect because it is essentially two-dimensional rather than three-dimensional in its principle.

This conclusion will be very much strengthened if it is possible to give an account of the conditions that explain the development of this third dimension as the product of complex relations, and it is to this task that we now turn our attention.

¹ An Optical Illusion, PSYCH. REVIEW, Vol. V., p. 286 seq.

The theory that space is a product of a complex of sensations is as old as Berkeley. Even before his time there were attempts of a rather crude type to explain space-perceptions as complexes. In all such attempts there has been a marked tendency to lay great stress on sensations of movement. Berkeley himself wrote: "This disposition or turn of the eyes is attended with a sensation which seems to me to be that which in this case brings the idea of greater or less distance into the mind."¹ The question of the relation of movements to visual perception of depth has been made the subject of elaborate experimentation. These experiments have dealt mainly with movements of accommodation. The earliest experiments were made by Wundt, and recently Hillebrand and Arrer have taken up the discussion and renewed many of the earlier experiments, making additions also in the way of methods and results.

The methods of these investigators may be subjected to careful examination. Wundt tried most of his experiments by allowing an observer to see with one eye a field of vision with a plain background and no objects other than threads which were suspended vertically in front of this background and were long enough so that they always reached from the top to the bottom of the visual field. The distance of the thread from the observer was varied, or else two threads were shown and the observer was called on to judge the relative distances. Hillebrand varied the method by using, instead of threads, mathematical lines which he was able to produce by taking the boundary between two surfaces as his object. In these latter experiments, as well as in Wundt's, the observer used only one eye, the other being covered or closed so that its retinal image was eliminated. In neither of these experiments, however, was it possible to eliminate movements of convergence. In spite of this fact, however, Wundt assumes that the convergence is not an important factor. Arrer tends more towards Hillebrand's position. Hillebrand is very explicit. He holds that the movements of convergence are in these cases the same as in normal vision, and that the results of the experiments apply, especially if they are negative results, to convergence as

¹Essay toward a New Theory of Vision, § 16.

well as to accommodation. The whole method is seriously complicated by the presence, in some degree at least, of movements of convergence in the closed eye. In *Science*, of February 25, 1898, I reported some experiments which showed that the movements of convergence which actually do take place are very complex. Until it can be definitely shown what are the results in sensation of such complex movements, it is evident that these movements cannot be ignored on the one hand or regarded as similar to those of ordinary experiences on the other. That the conditions of these experiments are exceedingly complex appears furthermore in the failure of the various investigators to agree on interpretations, even when their empirical data are the same.

The conditions for experimentation are very much better when we take up convergence rather than accommodation, and instead of trying to eliminate all other factors try rather to keep them constant. It can be shown in this way that movements of convergence are the sources of sensation which give rise to perceptions of depth.¹ But it is important to notice in this connection that, while it is shown that sensations of movement are factors in the perception of depth, it is not by any means shown that they are the only factors or even that they are the essential factors.

We turn to a criticism of the general type of theory which makes sensations of movement the essential factors of all space-perception. And, first of all, it may be argued that space is not identical with sensations of movement. This may be put in a still more general form: Space is not identical with any sensation quality. If we try to represent to ourselves absolutely empty space, we find that the nearest approximation to this which we are able to reach is a space which is only relatively devoid of content. It will always have some vague visual or muscle sensations, qualities in addition to its formal spacial attributes. Stumpf² has argued this point in detail, and we need only to accept his position as agreeing with the results of intro-

¹ 'Some Facts of Binocular Vision,' *PSYCHOLOGICAL REVIEW*, Vol. IV., p. 374.

² *Ueber d. Psych. Urspr. d. Raumvorst.*, p. 114.

spection. Space, then, always requires some content of which it is merely the form or attribute. But it is an essential characteristic of sensation qualities that they can be isolated from each other and represented as distinct. In the necessary relation of space to some quality, we have, accordingly, evidence that space is not itself a quality but rather in some sense a secondary factor. This general argument applies to sensations of movement. Sensations of movement are characterized by certain qualities of strain, but these qualities are not space, they are contained in space.

This leads to a second point. Why should sensations of movement be any more directly the sources of space-perception than any other sensation qualities? This question cannot be answered without finding in sensations of movement some special characteristic that will differentiate them from other sensations, and this special spacial characteristic, as we may call the undiscovered factor, must be something other than quality, for, as we have just shown, the quality of muscle-sensation cannot be identified with space. A reference of space to sensations of movement is not then a final explanation. This fact has been overlooked too often. Writers have been satisfied if they could explain every case of space-perception by showing that the sensation complexes which entered into them contain some factors of movement. Movement in some form has been regarded as a necessity because it has been held that this alone could explain the spacial character of the whole complex. This has led to the assumption of movement-sensations in cases where it is difficult if not impossible to show their presence. Take, for example, cases of binocular vision in which the two eyes are not moved at all. We are capable of very fine discriminations of depth under such circumstances. But it is answered, there have been movements in the past experience, and the result is that the retinal sensations at any given moment are supplemented by a large number of revived associations. This hypothesis is necessary if the importance of muscle-sensations is to be maintained in the cases where there are certainly no immediate sensations of this type present. The difficulty with this hypothesis is that introspection reveals no such revived muscle-sensation. Take

stereoscopic vision, for example. Here the third dimension of a body must, on the movement-sensation hypothesis, be due to movements of convergence. In the original and crude form of the hypothesis, it was assumed that these movements actually took place. It is merely a more refined form of the same hypothesis to say that they are associated sensations from convergence called up by the retinal images. The preception of the third dimension is certainly vivid enough, and one would think that the sensation factors which are necessary to the formation of these percepts must also be clearly perceived in consciousness. But this is not the case if movement-sensations are in reality such elements. For not only are movement-sensations not revived as conscious factors, but the keenest introspective analysis cannot succeed in bringing them out. Some observers may not agree with the statement here made, holding that they are conscious of such ideas of change in convergence. The difference in opinion is frequently to be explained by the fact that movements in the objects are mistaken for movements in the eyes. A plain figure that may be seen as a cone or as a hollow funnel may be used as a convenient figure to illustrate what is meant. As the object is seen passing from one position to the other, there is a very striking impression of movement, but it is the movement of the object, not of convergence.

Another strong motive which has always supported theories based upon sensations of movements is to be found in the fact that there is no other factor common to all sensation complexes which yields ideas of space. This common factor has been either consciously or unconsciously demanded in all explanations of space, because there must be some means of accounting for the fact that spacial attributes in percepts are always the same attributes whether they are connected with visual, tactual or other sensations. Take a concrete illustration of the difficulty that has arisen here. The blind man's space is primarily a space of tactual and muscle sensation. Normal space is predominantly a visual space. The most divergent opinions have been advanced on the question, Does the blind man's space correspond to that of the normal man? Or, take a case from ordinary normal experience. Does the space we perceive in the

dark differ in any way from the space we see when with perfectly stationary eyes we look at the same scene? The only possible answer to these questions seems to me to be that there is but one space whatever the sensations through which it is known. I am well aware that the contrary opinion is widely accepted. Berkeley's dictum on the subject is, "The extension, figures and motions perceived by sight are specifically distinct from the ideas of touch, called by the same names; nor is there any such thing as one idea or kind of idea, common to both senses."¹ Why, then, I ask, if these extensions are different, 'specifically distinct,' do we fail to recognize the difference between them? It is often answered that the visual and tactual spaces have become so thoroughly associated that they lose their identity. But it is not the function of association to identify associated contents. A and B are associated because they are similar or contiguous, but not because they are identical. The difference between association and space-perception may be well illustrated by looking at a rough surface. We associate certain tactual quality with these visual qualities, but we never fail to recognize the two groups of qualities as distinct from each other. The spacial attributes, on the other hand, like the object itself as distinct from its qualities, are the same whether perceived by touch or by vision. We may recognize the tactual content as distinct from the visual content, but both contents are referred alike to the same object and to the same points in space, and their extensions are recognized as identical. It was this fact that was brought out by the distinction that Locke made between primary and secondary qualities. Primary qualities must be known in the same way whatever the sense through which they are known. It is this fact which finds expression in the discussions that identify space and substance. The one factor common to all percepts representing objects is the spacial character of these percepts. The qualitative attributes vary indefinitely. This essential unity of space is one of the most important facts in the psychology of space-perception. Any theory which is to be at all adequate must be prepared to explain this fact. Explanation is, however, diffi-

¹ Essay, § 127.

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cult, because it is not easy to find in the various sensation complexes which give rise to spacial percepts any common factor. Some writers have found this common factor in muscle-sensation. But muscle-sensations, as already argued, will not serve to explain space even if it could be shown that they are always present, and there is much evidence which points to the conclusion that they are not present in all such percepts.

One other method of accounting for this unitary character of space has been the adoption of nativistic theories of various types. Take, for example, Lotze's discussion in the *Medizinische Psychologie*.¹ He writes: "Space is an original and *a priori* function belonging to the nature of the mind itself. It is not produced by external impressions; these serve merely to determine the particular application of the subjective function."

Such a solution of the problem is evidently no explanation of space. It merely refers space to the subject as an unanalyzable function and devotes itself to the discussion of the particular applications of this subjective activity. But here the old problem presents itself in just about the same form. Why should this subjective function apply to one content and not to another? There must be some attribute common to all of the sensation complexes to which this subjective function applies, and this common attribute must be present in various degrees and particular modifications, for not all sensation complexes are equally spacial in character.

This common attribute, which seems necessary, then, from any point of view, has been sought in all of the various theories in some concrete sensation quality. In place of any particular quality or qualities of sensation, there is good empirical ground for holding that the only common factor in all sensation complexes leading to spacial percepts is *a particular kind of relation*. The qualities may vary indefinitely. They may be derived from one sense or from several senses. They depend, however, for their spacial attributes not upon their character as quality, but upon the way in which they are related in the whole complex. *The common factor is not a concrete factor, but it is a relation.*

¹P. 335.

Let us test this hypothesis in connection with the problem in hand, namely, the visual perception of depth. We may assume the existence in retinal sensation of two-dimensional data, and raise merely the question, How can two-dimensional data be related so as to give rise to a percept with an entirely new attribute, namely, the attribute of solidity? Some of the experiments to which we must refer are familiar, but we shall review them briefly for the sake of systematic treatment. It is a well-known fact that two plain figures which are exactly alike may be fused in a stereoscope and that the result will be the perception of a single flat object. Absolutely nothing is added to the original monocular data. Complete identity of the two retinal images will result in a single two-dimensional percept. We may try to imagine solidity, but we can never perceive it directly. We may call up all the associations possible, and they will serve to make the imagined solidity vivid, but they will never result in a direct perception of depth. Suppose now, instead of these two figures that are exactly similar, we fuse in the stereoscope the familiar figures which will produce a truncated pyramid. The two figures differ in their horizontal attributes. This difference in horizontal attributes must not, however, be too great. If the inner squares are brought too near to the boundaries on the outer square, that is, if the difference between the two images is made too great, there will be no fusion at all. It is only slight differences that can be overcome. Complete identity of the two-dimensional attributes of the two groups of sensations is not demanded, but, on the other hand, the differences must not be too striking either in their degree or kind. If the difference is in the diagonal or perpendicular direction, even though it be relatively very small, it will, on account of its unfamiliarity, prevent the fusion of the two figures. Two groups of sensations with a similarity approximating identity, but with a slight difference in certain familiar directions, seems to be a full description of the conditions. This may be made somewhat clearer by a detailed examination of the figures. The broad side of the right figure and the narrow side of the left figure are the elements which combine in the completed percept. If these two sides were exactly alike they would fuse without

difficulty into a two-dimensional image. If the difference is too great they will not fuse at all. If the difference is slight they will fuse in such a way as to represent a side extending into the third dimension. *The third dimension is not a new content; it is a form in which the contradiction between the two-dimensional attributes of the two groups of sensations is eliminated.*

This description is continually hampered by the necessity of using terms which apply to conceptual rather than perceptual thought. Of course, there is no actual scrutinizing of the sensation data with a view to finding out the likenesses and unlikenesses, but there is an immediate synthesis of these factors. In order that they may be united into a single percept there must be some recognition of the differences as well as the similarities between the various factors. If two groups of sensation data belonging in the same system and disagreeing with each other are to be united, it must be possible to find some formal attribute, some relation between these factors, which shall permit identity and difference to be reconciled.

The experiment may be made very striking by covering up one of the sides of one of these figures after they have been fused in the stereoscope. The result is, first, that the corresponding side of the pyramid loses its third dimension. This is due to the fact that it is now a monocular object. Secondly, the width of the side thus seen as flat will be the exact width of the figure. It is impossible to get a symmetrical figure out of the pyramid. If space is the product of association this ought not to be so. The horizontal extension in the monocular figure means depth just as much when it is not united with the other monocular figure as it would in binocular vision, and if space is merely a matter of association, the associated movements should operate in this case to bring out the meaning as well as when the fusion with the other image takes place. But the side of the pyramid cannot by any possible turn of imagination be thrown out into the third dimension in such a way as to give a symmetrical image. The moment the covering is withdrawn so that the side fuses with the corresponding side of the other figure, all is different. The figure is symmetrical. The two sides of unequal width are no longer merely imagined as depth,

with horizontal extension based on the real width of the figure only, but they are fused into the third dimension, and this third dimension is an additional form of arrangement in which the contradiction between the two-dimensional attributes is eliminated and in which the width of the figure is interpreted in its full meaning of width and depth both.

The fact that the difference between the two images must not be too great can be illustrated by a variety of experiments. If we take in the stereoscope the two figures which produce a truncated pyramid and after fusion erase part of one or more of the lines in either of the figures, we find that the perception of solidity is immediately impaired even though it is impossible for the observer to determine which retinal image is incomplete. Or take the simplest possible figure, one made of dots placed at the corners of the larger and smaller squares, without any connecting lines in the figure anywhere. Such figures will fuse if the dots are similar in quality. Differentiate two or more of the dots by making one red and the other blue, and that part of the figure will no longer be solid. Or in such dotted figures draw one of the diagonal lines in one of the figures only. Again the figure will lose its depth in that particular part. And so on indefinitely, the groups of sensation factors must closely resemble each other or they will not fuse.

It may be objected that we are in these cases dealing with spacial attributes from the start, and are, therefore, begging the question by assuming two-dimensional data. But, as was pointed out in the earlier part of the paper, we can never hope to trace out completely all the processes by which space is developed in all its concrete phases. A single dimension added to data which did not originally possess it is just as much a development of space as would be the derivation of the first two dimensions. Furthermore, after reaching these conclusions in regard to the third dimension, we may take up the more difficult problem of the first and second dimensions, with better hope of final success.

This hypothesis covers the facts of movement-sensations as well as the facts of binocular parallax just discussed. Take the movements of convergence. The sensations of movements from

the two eyes differ slightly in quality and intensity. These differences enter into the total complexes of sensation, which are in the main alike, and give rise to spacial attributes. Their influence is not due to any qualitative attributes, but rather to the way in which the two groups of sensations coming from the two eyes are related.

In regard to accommodation the case is somewhat more complex, for here there are no simultaneous differences in sensations. The differences here are all differences between the memory image and the present sensation. Localization through mere changes in accommodation is possible, but this localization is very incomplete. There is only a relative localization, that is, a localization due to a recognition of the difference between a given state of accommodation and one recalled out of past experience. When a person is forced to make use of these factors to a greater extent than is ordinarily the case, as, for example, the one-eyed man, it may become a valuable means of discriminating depth.

Visual depth is thus conditioned by a certain relation of contradiction between the two-dimensional data of the retinal images or between the muscle-sensations which are connected with these images. The agreement between the elements in which the contradiction appears must be great, but it must not be complete, for the contradiction is an essential element. Depth, then, may be properly defined as a form rather than as a content of perception. It is not in any sense an original attribute of sensation. It is not especially related to any particular sensation quality. It is a complex form of perception in which any sensation quality which is presented in the proper relation may be arranged. It is furthermore an empirical rather than *a priori* form, since it depends upon the objective relation between sensations for its origin and character. It is, of course, like all psychical experience, subjective, but its source is just as much objective as is that of the sensation qualities which are arranged in space.

DISCUSSION AND REPORTS.

PROFESSOR TITCHENER'S VIEW OF THE SELF.¹

My object is to discuss Professor Titchener's view of the self—as revealed in his 'Outline of Psychology.' I wish to do so in as positive and non-controversial a spirit as possible, working toward a positive psychology of the self, through what I most respectfully beg to call the limitations of Professor Titchener's treatment. The positive value, to be sure, of Professor Titchener's 'Outline' is entirely unaffected by what I shall say. His book is the clearest and most instructive book upon the newer psychology that I, at least, know of. Were it my business to teach experimental psychology, I should try to do so through its use. And were I meantime reviewing the book, I should devote several paragraphs to the enumeration of such elements of positive value as I am capable of seeing in it.

In regard to the self, there are two lines of treatment in the book, one of positive presentation, and another of discussion of hypotheses. The first is indicated by way of summary, on pp. 287-292: the self is the "sum total of conscious processes which run their course under the conditions laid down by bodily tendencies." The second is the denial that there is any psychological evidence of 'a mind behind mental processes,' or of 'a mental activity above or behind the stream of conscious processes,' or of 'mental continuity and coherence.' My contention in regard to the first part, in regard to Mr. Titchener's purely positive treatment of the self, is that it is all good enough as far as it goes, that it must all be incorporated into a complete psychology of a self. It *may* (although I shall question even this much) represent the point of view of psycho-physics, but it does not completely represent the point of view of psychology (and Professor Titchener's volume is called an 'Outline of Psychology'). In regard to the second point, the discussion of hypotheses about the activity and nature of the self, I beg to remark: (A) Is not the very fact that Mr. Titchener in his 'Outline' has felt himself called upon to discuss theories about the ultimate nature of the self, itself a fact of psychology, or even of psycho-physics? And

¹ Paper read before American Psychological Association, Ithaca, N. Y., December 29, 1897.

is not the belief that humanity has in the active and permanent character of the mind a thing that Mr. Titchener's positive psychology cannot account for? (B) While Mr. Titchener's contention that questions about the ultimate nature of mind cannot be answered until 'we have brought together the facts of psychology and the facts of other sciences,' is a welcome admission as coming from a psychologist, and one that ought to be proclaimed, as it were, 'from the house-tops,' it cannot be said that he himself has incorporated this admission into his psychology. Why is psychology alone unequal to the task of setting forth the nature of mind? Surely, because it *sees or encounters* some things in mental processes that *cannot* be explained as mere processes. Professor Titchener's procedure and conclusions place ethics and logic, and aesthetics and metaphysic in the ridiculous position of building up theories of mind 'in the air,' on ground which is no ground, on ground whose merest existence is denied by psychology, the basal mental science, the science that enumerates the facts of mind, and from which other mental sciences consequently borrow. I am aware that Professor Titchener has chosen his subject out of the various divisions of psychology—anthropological, social, comparative, and so on—the psychology of aesthetics, the psychology of ethics, etc. Describe it as we will, his subject is psycho-physics. On the ground of psycho-physics he finds in 'mind' nothing but sensations and affections, expressly denying a 'third' element, such as 'activity' or an 'ultimate' mind or 'will' working from within outwards, conditioning mental process. I differ from Professor Titchener in so far as I think that, *even along the lines of psycho-physics*, we do come upon mental activities or a *tertium quid* different from sensation and affection, a selfhood that psycho-physics itself cannot explain, but is compelled to assume. Psycho-physics must confess to philosophy that it *has* discovered some things in the psycho-physical mechanism which cannot be explained as sensations or affections, or any combination of sensation and affection, which cannot be explained by empirical psychology with the help of biology (as Mr. Titchener uses it). I know and see that Mr. Titchener is an exact scholar. I know that he himself again and again says and implies that, of course, sensations and 'affections' are never encountered as such, pure and simple and self-existent. I know that he expressly says and implies that the simplest working element in experience is the 'idea,' etc. My contention is that he has not made enough of this admission and of other admissions which he must consequently make. To do so would give a different tone and color to his book.

Were he now to reconsider the point, it might perhaps have some effect upon his future 'Principles of Psychology,' in two volumes, which we shall look for from him.

I do not wish to enter upon the question of how far Mr. Titchener's mere language about the self is sometimes (as on pp. 287-8) perfectly satisfactory, or as to how far it sometimes indicates a willingness on his part to have us add on to his account of the self, facts which we may learn from other sciences and from philosophy. And I repeat that the didactic part of his book is occupied with the attempt to show how sensations and affections and tendencies constitute the working-life of the normal mind, or self, and that for this alone we ought to be grateful to him. But I object, in the name of psychology and philosophy and experience, to his practical contention that from the standpoint of psychology there is in the self nothing other than sensation and affection. I think that introspection makes us aware of an activity in the self which is not explained by Mr. Titchener's psychology of effort and attention and voluntary movement, and that there is in us a feeling consciousness of selfhood, or the realization of (as Aristotle puts it) an inward operative (formal or final) purpose which is not expressed by his biological self, his 'sum of tendencies.'

Some of my reasons for proclaiming his account of the self to be faulty, even from the standpoint of psychology alone, are as follows : (1) His analysis of effort or conation (intended by him for a deliberate and complete induction) is not an analysis of effort at all. It is an analysis of *desire*. "I do long to go to Italy;" "I do wish it was dinner time;" "If I only could remember that name" are all (p. 123) states of desire and not experiences of 'effort.' I venture to call these important sections an 'oversight' on the part of Mr. Titchener, important though they are in other respects. He has *not* examined 'every instance' of effort, although he claims to have done so in these very words. It is true that his analysis of effort is supplemented by an analysis of attention, although, as I say, he claims on page 123 to have shown that there is nothing in effort but an affective quality and complex of sensations. (2) The fact remains that his examination of effort and attention is a *dogmatic* examination, one that is influenced by a certain preconceived method. He says on page 124 that "the rules which we possess for the use of introspection apply only to the examination of these two processes" [*i. e.*, sensation and affection]. He approaches the study of effort and attention with an *apparatus belli* which will enable him to see in these experiences only the qualities of sensation and affection. He asks (just as Münsterberg¹ does

¹ *Die Willenshandlung*, Ch. II.

about the sensations that constitute (!) will about the quality and intensity, etc., of the attention-process, as if attention were only a sensation (or affection) like to other sensations in having these very sensation characteristics—quality, intensity, duration, etc. True, he says on page 135 (and of course he knows) that attention is a *complex process*, but he does not seem to examine it as such. One looks in vain in Titchener for a recognition of that 'prospective attitude of mind' and that 'schematic apprehension,' and so on, that Stout finds in attention. As Stout¹ says of James, so might we say of Titchener (as indeed Stout indicates in a notice² of Titchener's book), that he is 'identifying the activity of the self with certain particular items of our conscious experiences to the exclusion of certain others.' If Wundt and Ward in treating of 'attention' separate 'activity' from 'content,' so Titchener surely separates the 'content' from the 'activity' of attention. To be sure, both Mr. Titchener and myself are here upon dangerous ground. A thing is nothing apart from its qualities, and Mr. Titchener in pointing out certain psycho-physical attributes of attention has done scientific service. But he need not deny the activity itself in favor of the attributes, especially in view of the fact (conceded by him elsewhere) that the simplest item of our experience is not sensation or affection, but the 'idea.' If the 'idea' be the simplest item in our experience, then what Wundt calls apperception, or Spinoza a self-preservation nisus, or Lloyd Morgan control, or Schopenhauer or Scotus Novanticus will, and many psychologists 'activity'—an activity that, with Ward say, is in advance of the actual elements that enter into our experiences 'from without'—is the key-note both of 'attention' and of the 'self.'

(3) Mr. Titchener distinguishes the 'activity-inference' from the 'activity-experience.' It is true that much that we affirm about the activity of the self (in itself or in attention) is inferential. It is true, as Titchener insists, (α) that we have no experience of the release of a voluntary movement, and (β) that active cannot be distinguished apart from passive attention, and (γ) that all attention is partly determined by factors and elements outside immediate experiences. And it is good to have these things explained. I agree with Bradley that the current use of the concept of mental activity is a 'scandal'; it tends to perpetuate error and ignorance and false educational methods. But I still believe, with a great many psychologists and philosophers, in the activity-experience. It is the experience that

¹ *Analytic Psychology*, I., 143.

² *Mind*, July, 1897.

gives rise to the inference and not *vice versa*, as Titchener implies. As we read Titchener we see how 'psycho-physical process' is moulded by 'tendencies' and 'dispositions' and 'associations.' But Titchener forgets that psycho-physical process has itself moulded tendencies and dispositions and associations, and that, as Stout¹ says, it "is 'self determining' in a double way: (1) in so far as it initiates the changes on which its propagation depends; (2) inasmuch as the brain substance, in which these changes take place, has been rendered capable of them only through previous psycho-physical processes in which it has taken part." Because mental activity is conditioned by all the laws and conditions of the universe, it is none the less mental activity; on one showing of things it may be *passivity*, on another it is none the less *activity*. Titchener will not call it both active and passive; he insists that it is passive, consisting merely of sensation and affection. If attention involves the 'idea' it involves *appception* and *judgment* and the so-called higher mental processes, control, etc. Ribot finds that will as choice involves judgment. In choice we affirm a certain experience (perhaps a future state of feeling) to virtually belong to the self, to be bound up with it, to be the ideal self (as different from the present self), to be bound up with the real self. But, as Stout contends, we cannot think of the self without affirming it. There is thus an active, or judging, or self-asserting self bound up with the higher forms of attention.

(4) Mr. Titchener does not think of this self that affirms itself, because he is concerned in the main with the psychological self, and leaves out of count the 'moral self' and the 'epistemological self' and the 'ideal self'—things that, e. g., Mackenzie recognizes in his *Mind*² articles upon Bradley's view of activity. He refers, indeed, to these other 'selves,' but his procedure makes us feel that none of them has any psychological basis, there being, in his eyes, no activity, but only 'tendencies' bound up with the self. Now, they have, all of them, a psychological basis. One thing alone in *attention* would prove this—the *time* or *duration* experience, in which a present element of our experience is denied, and a possible future element is affirmed. I think that the mere time-experience in attention, the waiting, the 'intention,' actually brings along with it a judgment experience, an apperceptive or ideal self that affirms and asserts itself. The self as 'ideal,' of course, lies 'ahead'³ of us, and not 'behind' us, as does the merely

¹ *Analyt. Psychology*, I., 155.

² July, 1898.

³ As Bosanquet puts it, in his *Psychology of the Moral Self*, its reality is always teleological. See *Phil. Rev.*, Mar., '98. Aristotle's view is the same.

'biological' self of Titchener. But the higher self or the higher forms of attention are just as much aspects of the attention or volition process as are the simplest forms of reaction to stimulus from without. Ideal ends are sometimes sought and realized at the expense of the lower or biological self. In the higher forms of attention the self affirms itself as action. And this is matter of psychology. Psychology must allow for the activity experience. (5) Many important elements or phases of the self that are distinctly matters of observation or experience, and that are noticed too by other psychologists, are omitted by Mr. Titchener. Wundt in his *Psychology* seems to speak of will as the highest kind of psychical formation and of willing as the fundamental fact of mind, things that we naturally expect from the emphasis laid upon *apperception* both in the *Logik* and the *Physiologische Psychologie*. He says that the feeling of activity which accompanies willing is closely associated with¹ "an immediate feeling of the interconnection of all individual psychical experiences." Titchener gives no definite or extended recognition to this immediate feeling of activity or of the interconnection of all separate psychical experiences. Nor does he allow for that conscious control of all the reflex and automatic and semi-automatic activities of our life which is an undoubted psycho-physical fact, and which seems, as Lloyd Morgan² puts it, to be accompanied by a psychical feeling of activity altogether above the ordinary strain feelings of mere muscular 'effort.' (This 'effort,' to be sure, is evidently just the compound of *sensation* and affection that Titchener finds it to be.) (6) I find the same absence of unity in Professor Titchener's conception of the biological self that I do in his conception of the psychological self. Foster in his *Physiology*³ refers to an ultimate tendency of living matter to act from within outwards, to act in a way that cannot be explained by mechanical or reflex action. There is a self-preservation *nissus* in the life of a biological organism which may be said to constitute its unity, to make it more than the mere sum of tendencies. And, similarly, there is in psychical life an effort after the unification of our experience, after the assertion of an ideal or personal or enduring self. If this effort is not to be called activity I do not know what to call it. Professor Lipps, of Munich, in a paper before the International Psychological Congress of August, 1896, spoke of the self as having its root not in a complex of sensations, but in the *immediate experience of volition*.⁴ In short, we have psychology and

¹ See *Mind*, Oct., '96.

² *Introd. to Compar. Psychology*, Chap. XI., etc.

³ Vol. I., p. 282; also p. 289.

⁴ See Report of Congress. *Vierteljahrsschrift*, etc., 1896, p. 486.

philosophy (Spencer and Schopenhauer and Aristotle and so on) and biology and physiology and experience all bearing testimony to the fact of an organic or self-preservative or unifying effort or desire or movement as constituting the ultimate nature of the self—*i. e.*, as far as we are entitled to talk about the ultimate nature of anything. If Professor Titchener would grant us even that *desire*, or tension, or ‘impulse, akin to desire, constitutes perhaps the last word that we can say about the distinctively psychological self (for desire includes the ‘idea, and ‘movement’ and ‘affection’), I should not be questioning his ‘first principles,’ as I am now doing.

(7) Mere ‘presentationism,’ as Ward and Laurie,¹ Andrew Seth,² and many others insist, is not a complete psychological account of the self. That is, the sensation and affection that we can *objectively see* in so-called ‘activity’ and ‘attention’ are not all that psychology tells us about the self. The persistency of the notion or illusion of a ‘spectator’ or self, for which all mental processes occur and which unifies all mental states, is of itself evidence that there is psychological fact beneath it. If the self were merely the creation of an inference it would not be the belief that it still is; it would disappear. It is contrary to all that we know about the workings of nature to suppose that she would perpetuate the existence of a mere ‘epiphenomenon’ such as a ‘consciousness’ whose function is not *activity*—guidance, interference, in our life. (8) Another reason for Mr. Titchener’s inadequate account of the self is his inadequate treatment of feeling. Feeling is not merely the subjective side of sensation that he makes it out to be. Feelings are excited by inward as well as by outward activity, and certain feelings, such as those of sex and the sociological fact of imitation, point to an inward activity and unity of the psycho-physical mechanism that are evidence of an active or organic or unifying self or will in man. Should not ‘emotions,’ too, be referred to the self as well as to ‘objects?’ Are there not emotions that are peculiarly ‘personal’ or ‘subjective,’ *i. e.*, whose existence implies that of an active self, or a mind that persists through ‘process?’ (9) The phenomena of ‘the unconscious’ or of unconscious action are, it would seem, overlooked by Mr. Titchener. These phenomena all point to an inward activity in the organic self that is at least as ultimate as are sensations and affections. The first and simplest sensations are, as Schneider³ points out, sensation-impulses. Indeed, from

¹ *Mind*, Jan., 94.

² *Man's Place in the Cosmos*. ‘The New Psychology,’ etc.

³ *Der mens. Wille u. s. w. passim.*

the biological point of view, our senses of sight and hearing and taste and smell are the creations or developments of the life-preservative efforts of the organism, of efforts that emanate from within. (10) Mr. Titchener's psychology of consciousness is defective. Consciousness is not the 'sum of mental processes'; it is the awareness of these processes as a sum. Consciousness is not a 'cross-section of mind.' It is the active feeling of our life as a unity, which enables us to look upon any part of our experience as a part or 'cross-section.' (11) Mr. Titchener forgets (what Wundt remembers in his Psychology) that a psychical formation is a very different thing from the elements that constitute it or the properties of these elements. He explains the highest psychical formations from their lowest elements; it is equally important for psychology to explain the lowest elements from the point of view of the highest psychical formation, such as control and conduct and self-affirmation. Mr. Titchener does not allow for his admission (p. 5) that in the 'origination and continuance' of mental processes we 'are ourselves necessarily concerned.' In these words he admits the 'we,' the 'I' to be fact psychological. (12) In conclusion, Mr. Titchener's treatment of metaphysic might be discussed. My time-limit, however, forbids this. As to his own philosophy, he is always insisting that mind is nothing apart from its processes. He does not allow for the fact (the unique thing in psychology) that psychological happenings and processes are nothing apart from an active, unifying, synthetic self. 'Self' is doubtless a 'mystery'—an 'ultimate'—but there are psychological manifestations of its reality.

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A TEMPORAL ALGOMETER.

The temporal algometer is intended to measure sensibility to painful or rather disagreeable impressions caused by pressure, and it is generally applied to the temporal muscles. The instrument consists



FIG. 1.

of a brass cylinder BF, with a steel rod C running through one of the ends of the cylinder. This rod is attached to a spring, with a marker

E on the scale A ; this scale is graded from 0 to 4,000 grammes. The brass disc D is 15 millimeters in diameter ; a piece of flannel is glued to its surface, so as to exclude the feeling of the metal when pressed against the skin, thus giving a pure pressure sensation. The whole instrument is 30 centimeters in length.

In using this algometer it is held in the right hand at B, by the experimenter, who stands back of the subject and presses the disc D against the right temporal muscle, and then he moves in front of the subject, where he can conveniently press the disk against the left temporal muscle.

As soon as the subject feels the pressure to be the in *least disagreeable* the amount of pressure is read by observing the marker E on the scale A. The subject sometimes hesitates to say just when the pressure becomes in the least disagreeable, but this is part of the experiment. The purpose is to approximate as near as possible to the threshold of pain.

In making experiments upon both sexes, women were found to be more sensitive to pain than men.

The instrument is made by Charles Verdin, 7 rue Linné, Paris. Price, 60 francs.

U. S. BUREAU OF EDUCATION,
WASHINGTON, D. C.

A. MACDONALD.

'SOCIAL INTERPRETATIONS.'

The interesting remarks made by Professor Tufts in his kindly notice of my volume on 'Social and Ethical Interpretations' in the last number of this REVIEW might profitably have extended comment. I find it difficult, however, to be sure from the condensed statements of Professor Tufts as to the exact bearings of his criticisms; and hence I shall at this time only make a general statement or two.

First, regarding the 'general' and 'ideal' self, which he thinks is not clearly enough defined in the book, he asks (p. 318): "Is the social or general self the outcome of the dialectic in such a way that both the *ego* and the *alter* must enter into it, and become *as such* elements of it, or is it conceived as merely the undifferentiated raw material out of which *ego* and *alter* develop, but which does not include them?" He adds: "Perhaps the note on p. 266 means that both the above alternatives are true and represent successive phases in the development of the social self."

In answer to this question I may say that Professor Tufts' surmise regarding the note on p. 266 is quite correct; the note was added to make it clear that the alternative phrases used in the text at that point

(‘general’ and ‘ideal’) referred to the same content looked at from the two points of view of what is ‘undifferentiated,’ on the one hand, and as the outcome of the dialectic into which both ego and alter must enter, on the other hand. The former is the ‘ideal’ self considered as having a ‘projective’ value, a something-over not realized in actual self-experience. The latter is the ‘general’ self, considered as including what is common to ego and alter at any particular stage of progress of the dialectic of personal growth. This latter is what I mean by the ‘social’ self when speaking of it as an organized thought. The general self is always ‘social.’ So also is the ‘ideal’ self considered as to its actual content, which is, as I said above, the content of the general self; but in so far as it is ideal it stands for the further projective something-over, which is not yet organized in experience. In short, the ‘social self’ is at once a ‘general’ self and also, by the continuance of the dialectic, the bearer of the ‘ideal’ values. It is the meaning and the peculiarity of the ‘projective’—and this made it necessary for me to adopt the word—that there is this sense of value or worth keeping ahead all along of the actual growth of the ‘general.’¹

So I am astonished when Professor Tufts goes on to say that I do not do justice to ‘conceptions of value.’ The whole treatment of the origin of social judgment to which the earlier chapters of the book are devoted leads up to the social determination of ethical values. Social judgments of worth are the important things all the way through. The recognition of social approval, of the social criterion, etc., is a distinctive feature of my work. I hold the child and the genius alike, the moral informer and the social propagandist alike, close down to social tests of worth I fear in this—if I understand him—Professor Tufts has missed the forest for the trees. Possibly Professor Tufts construes what I have said about ‘suggestion’ in this matter exclusively under the heading of ‘law and authority,’ but it was not so meant. It is only in connection with law and sanction that I emphasize the parents’ authority.

In the remarks on the absence of the ‘value’ element in cases of spontaneous desire and ethical sanction, however, I think there is a real difference between Professor Tufts’ views and mine, which I cannot go into now. Part of the difference may be due to different uses of the term ‘end.’

¹ As for ‘hoping, fearing,’ etc., for ‘self in general,’ neither do I believe the child does it, and I am revising the passage (p. 16) with others in which slips in expression have occurred.

In regard to what I have called 'reflective bashfulness' I am convinced by various reports from correspondents that my own children developed earlier than many do in this respect. In new editions of both my volumes I am giving 'three years and later,' instead of 'in the second and third years,' in describing this epoch. As this 'reflective bashfulness' is what goes on to develop into self-conscious modesty, its existence sooner or later cannot be in question. The point on which more light is needed is as to the existence of an intermediate period of relative friendliness—which both my children showed—between the earlier and the later exhibitions of bashfulness.

In conclusion I may especially thank Professor Tufts for the subtle compliment implied in the words: "But I am convinced that few children develop in such a favorable moral atmosphere as that of the children observed by the author!"—that is if he do not spoil it by saying he did not know the children were my own!

J. MARK BALDWIN.

PROFESSOR MÜNSTERBERG ON 'THE DANGER FROM EXPERIMENTAL PSYCHOLOGY.'

In an article with the above title in the *Atlantic Monthly*, for February, Professor Münsterberg maintains three theses, from all of which I dissent. These are: (1) that a recent book by Dr. Scripture is 'a climax of blunders'; (2) that mental processes cannot be measured; and (3) that experimental psychology is useless to the teacher.

Dr. Scripture's 'The New Psychology' seems to me to be a good and useful book, and this is the opinion of competent reviewers in several journals. The book, it is true, covers only a part of psychology in which the author is especially interested and to which he himself has made valuable contributions. But such monographs are, perhaps, more needed at the present time than compendiums of the whole science. As a matter of fact Professor Münsterberg's only criticism of the book is to the effect that it claims to give the results of psychological measurements and classifies them under the magnitudes measured. I think that I am to some extent responsible¹ for the method of classification followed by Dr. Scripture, and I am quite prepared to defend it. Professor Münsterberg himself uses the magnitude measured in naming his separate articles 'Zeitsinn,' 'Augenmass,' 'A Psychometric Investi-

¹ *Mind*, 1888; *Philosophical Review*, 1893; and elsewhere.

gation of the Psycho-physic Law,' etc., and in giving a review of such experimental work the same classification can be conveniently maintained. Classification is a matter of convenience, all our classifications being to a certain extent arbitrary and artificial. Thus, when mental processes are classified into cognitions, feelings and volitions, or into sensations and perceptions, the student is apt to get an idea of sharp distinctions that do not exist. In a system of psychology an author will naturally use the method of classification that he regards as best expressing his system; in an exposition of laboratory work an objective classification has many advantages, both practical and theoretical. Certainly objections may be urged against such a classification, but Professor Münsterberg's article will scarcely tend to a solution of the problem either for the psychologist or for the lay reader.

Professor Münsterberg has published numerous measurements made by a psychologist in a psychological laboratory for the analysis of psychological processes. Whether these should be called physical or psychological measurements is to a certain extent a matter of nomenclature. The epistemological questions involved are important, but are such as psychology as a natural science can ignore. They would be quite unintelligible to most readers of the *Atlantic Monthly*, and have no connection with the value of experimental psychology to the teacher. When Professor Münsterberg says that the unmeasurable character of psychological facts is a fundamental presupposition of psychology, he must mean of his own system of psychology. Most psychologists will agree with Kant that mental processes have time as a dimension. Otherwise how is genetic psychology possible? And if Professor Münsterberg thinks that the time of mental processes cannot be measured, why does he entitle one of his papers 'A Psychometric Investigation, etc.'? Whether mental processes have a measurable intensity and extensity is a complex question which I have elsewhere discussed and to which I intend to return. Professor Münsterberg has himself said, and in italics, '*eben weil die Grundlage dieselbe ist, kommt der psychischen Intensitätsmessung auch dieselbe Berechtigung zu wie allen physikalischen Messungen.*' (*Beiträge zur exp. Psychol.*, 3, p. 23.)

Professor Münsterberg says to the teacher: "This rush toward experimental psychology is an absurdity. * * * Do not expect that it will help you in your work as teachers more than astronomy or geology would help you," etc. Now, the practical importance of experimental psychology for the teacher has doubtless been overestimated on occasion, but this scarcely seems an adequate reason for writing in a popu-

lar journal that it has no use whatever. Professor Münsterberg and I probably agree in holding that experimental psychology is not a separate science, but a method in psychology. I fail therefore to understand, in spite of the further explanation in the *Atlantic Monthly* for June, why the experimental method is thought to be of no use to the teacher, unless it is also of no use to psychology. I myself believe that the experimental method and its results have been useful in many ways, among others in making psychology more real and profitable to the teacher. I think that psychology has much the same relation to the profession of the teacher as physiology has to medicine. Teaching, like medicine, is an art dependent largely on insight and natural aptitude, but it is well both for the teacher and for the physician to know well one science, and for each to choose that science most nearly related to his profession. Professor Münsterberg opened his first article published in America with the sentence: "The experimental study of memory, important both for psychology and for pedagogics, is, as yet, only begun." This seems to me a more correct attitude than that of the *Atlantic Monthly* articles. The experimental study of the senses, of memory, attention, habit, fatigue, etc., has enriched psychology in a direction of special interest to pedagogy. The practical applications are limited, but we hope that they will increase, and in the meanwhile the subject is profitable and stimulating to the teacher.

J. McKEEN CATTELL.

PSYCHOLOGICAL LITERATURE.

A Course in Experimental Psychology. By EDMUND C. SANFORD,
Assistant Professor of Psychology, Clark University. Part I.:
Sensation and Perception. Boston, D. C. Heath & Co. 1898.
Pp. 449.

The final appearance of Dr. Sanford's 'Course in Experimental Psychology,' even though the volume is to be known as Part I., is an event which psychologists, and particularly American psychologists, are certain to greet with pleasure and approval. Nothing of this kind seems to have been attempted as yet in any other land, and, although Professor Cattell has for some years announced the preparation of a similar handbook, and Dr. Scripture has recently published some laboratory directions, the honor of producing the pioneer work in this field belongs to Dr. Sanford. The need for such a laboratory manual is probably a distinctively American need, for in this branch, as in others, American educational methods insist much more thoroughly upon systematic guidance and direction of the student than is customary in other lands. The practice course which aims to give an intimate acquaintance with the methods and material, the fundamental facts and demonstrations of the science; in brief, the use of the laboratory as a pedagogical influence rather than as a research-room, is an institution which promises to become increasingly popular and useful in our colleges and universities. It is gratifying to record that the psychological laboratory is now recognized as worthy of equal consideration with those connected with other sciences, both in intrinsic interest and in educational value. The same can hardly be said regarding completeness and appropriateness of equipment, or a highly desirable consensus as to what is fundamental in method or content. The recentness of the establishment of the psychological laboratory as an educational factor is a sufficient excuse for this shortcoming; but the defect cannot be outgrown by time alone. It requires time and wisely directed effort. It is as the most distinctive contribution to such effort, wisely directed, that Sanford's 'Course in Experimental Psychology' merits widespread recognition.

The volume consists of upwards of three hundred pages devoted to the senses, with an additional hundred pages containing a chapter

on the psycho-physic methods and a chapter on apparatus. Each chapter is supplemented by a well-selected list of references for further study. The senses are taken up in the order of 'The Dermal Senses' (20 pages); 'Kinesthesia and Static Senses' (18 pages); 'Taste and Smell' (5 pages); 'Hearing' (31 pages), and 'Sight' in three chapters, containing in all 224 pages. The three chapters are devoted to an account of the mechanism of the eye (38 pages), to the sensations of light and color (45 pages) and the several factors of the visual space perceptions (141 pages). The second part of the volume is to contain chapters on 'voluntary movement, memory, attention, emotion and other complicated mental states, in so far as they are open to experiments of moderate difficulty.' This distribution of topics is interesting as reflecting the perspective of the experimental aspects of the subjects considered. It indicates that it is in regard to the fundamentally acquisitive mental functions, connected in many cases with well-determined physical and physiological processes, that experiment must ever find its most suitable material. The student will hardly derive from this emphasis an exaggerated notion of the importance of sensation in the mental life, for it is the instructor's province to profusely illustrate and interpret the experiments in the light of the perceptive and assimilative processes which they so largely involve; for, as the author remarks, "it is evidently impossible to take out any sort of mental phenomenon for entirely independent examination," and the instructional course which accompanies the laboratory practice is the place where the original setting of the sense experiences must be properly delineated. The point of view determines the result quite as much as the composition of the scene, and whether these experiments become mainly a special physiology of the senses, or a detailed course in the peculiarities of human sensation, or a general illustration of psychological processes, will depend upon the skill of the artisan. The material has been judiciously selected, well arranged and intelligently presented; different builders will use it very differently.

The author himself acknowledges that it is likely that he offers 'a superfluous liberty of selection;' the *embarras des richesses* is in itself no disadvantage unless it leads to confusion and an obliteration of fundamental principles by a multitude of details. The simplest apparatus that illustrates the principle (considerations of accuracy are disregarded for the moment) is the best, and the simplest experiments should in some way be assigned a more fundamental place than the others. In the pursuit of fifty pages of optical illusions even a very discriminating student may lose his way without careful guidance between the

highways and the byways. This defect, if defect it be, increases the responsibilities of the instructor, and urges the necessity of differentiation between fundamentals and accessories, as well as between a mere description of an experiment and its interpretation.

Viewed in its entirety the volume must unhesitatingly be pronounced to be a highly successful achievement of a highly difficult task. It is doubtless a much simpler as well as more agreeable undertaking to prepare a text-book in psychology than it is to sift out from endless sources and combine into a substantial compilation the facts upon which so much of psychological interpretation rests. It is particularly difficult to undertake this without guides or precedent; the successful achievement is the more notable by reason of the inherent difficulties. The present writer has used the manual in various stages of incompleteness as it appeared in the *American Journal of Psychology* and in advance sheets, for about five years in practical class-work and demonstrations, and is thus able to add to his appreciation of the scholarship of the compilation his verdict of its practical utility.

JOSEPH JASTROW.

Handbuch der physiologischen Optik. H. VON HELMHOLTZ. Zweite umgearbeitete Auflage. Hamburg und Leipzig: Leopold Voss. 1896.

This is a very difficult book to review. Whenever one takes it up it is impossible not to be struck by some fascinating page which one has either not read before or not fully felt the meaning of, and that is an end of the critic's state of mind. But, in fact, there is no need of putting oneself into the critical frame of mind in noticing the new edition of the *Physiologische Optik*. With the completed volume before one, nothing but admiration can be felt for this model of the scientific method and the scientific spirit which has set a standard not easy to be reached again by the master of any science. Professor James has given expression to the common feeling in regard to this great work when he speaks of it as "a book which, on the whole, I imagine to be one of the four or five greatest monuments of human genius in the scientific line." The term genius is well chosen, for genius may be taken as connoting not simply great intellectual powers but a fusion of the quality of greatness in every element of the human being to whom the epithet is applied. It has lately been said by an acute observer that there is a common quality in the work of Darwin and that of Helmholtz, and a quality which is not only of the intellect, but of the spirit as well. Both writers have not only a complete mastery

of their subject in all the ins and outs of its ramifications, but they have also that tone of mind which one comes nearest to describing if one calls it *repose*—that mental atmosphere which we recognize as natural in one who has gained mastery of execution in any form of art, but which we are not so prone to think possible in the intellectual workshop.

The first edition of this great work was issued between the years 1856 and 1867; the second has also been exactly eleven years in coming out (1885-1896). For the first edition, Helmholtz undertook to work over, either by experiments of his own or by repeating the experiments of others, all the important points discussed; for the second edition a correspondingly enormous amount of labor could not be undertaken, but nevertheless it was designed that every important memoir that had appeared in the interval should be carefully considered, so far as it seemed to be an advance upon, or a desirable confirmation (or, as it might be, a refutation) of the results and opinions originally maintained. This plan was adhered to for the three parts first issued, but then the author's interest in the subject became aroused with something like its original intensity, and he began again to work at the solution of particular problems, without, indeed, experimenting himself, but basing his conclusions upon the experiments and measurements of others. In this way several memoirs were issued which were then, combined in part with the work of others, transferred almost literally to the out-coming volume. A notable paper of this category is the one on the connection between the external world and the data of the sense-organs. When the first edition of this work came out the doctrine of the empiricists had not been fully worked out; in the preface to that edition Helmholtz points out that other writers on this subject had treated only particular aspects of the question and that their views had not unnaturally been full of conspicuous contradictions; being himself of the opinion that, in a region in which disorders and disconnections prevail, even an untenable fundamental principle may have a useful function to perform in the marshaling and coördinating of facts, he gave a full exposition of the empirical theory, and in the course of this train of thought he became more and more convinced that it was the only one which could serve as an adequate guide through the wilderness of already known facts. In the present edition he holds to the same view with equal conviction, and his exposition is an admirable presentation of the best that can be done for that view. Nevertheless, in spite of the fact that the observer of each new case of a totally blind person who has

been restored to sight gravely states that his case is a fresh confirmation of the empiristic theory, and in spite of the fact that the nativistic theory as expounded by Hering offers nothing that is to be preferred to it, it is certain that v. Helmholtz's view has quite lost its hold upon sound-thinking psychologists, and especially since the acute discussion of the subject which has been carried on in more recent times by Stumpf and James and Külpe.

It must also be admitted that the theory of the light-sense (as distinguished from the space-sense), which v. Helmholtz developed and defended, is considered at the present time by most of the psychologists to have very little in its favor. It is true that this is to be accounted for in part by the fact that the argument against that theory is patent to any one who is in the habit of rigorously analyzing sensation, while the argument for it is largely of a rather abstruse mathematical nature, and is not easily accessible to one who does not approach it with mathematical training. (So far as I can make out Dr. Scripture is the only pure psychologist who has given evidence of understanding the real force of this argument; nevertheless, it involves considerations which, as Dr. Scripture points out, are not elements of a theory merely, but plain—and most important—matters of fact.) In this case also the victory seems not to be likely to go to the other of the two great contestants. Hering's theory, especially so far as it is a theory of assimilation and dissimilation, has always had to hold its own against a strongly antagonistic inertia on the part of the physiologists, and it shows no signs of overcoming that difficulty. Although the theory of Wundt has not proved particularly attractive to the attackers of theories, it is probable that by actual count it would be able to exhibit a greater number of adherents at the present time among those who are actively interested in the subject of theories (and these are, of course, the only ones whom it is worth while to take account of) than any of the others.

It is probable, therefore, that the two great doctrines which are most intimately associated with the name of Helmholtz, in the region of physiological optics, are destined to be given up in the near future, if they cannot be regarded as having been given up already. But what of that? It is the common fate of doctrines to be superseded by others better fitted to meet the needs of the time, and in this case the change of sentiment in these two particulars does very little to invalidate the claim of this book to be the indispensable and constant companion of whoever would concern himself with the subject of which it treats. It is a vast store-house of far-reaching, minute and

thoroughly tested knowledge, and it will be very long before there is anything to take its place. In fact, one is sometimes brought to think that it is much too comprehensive; at least one poor writer for a German scientific journal has complained that he cannot get his contributions attended to because every one puts him off with the words: 'Es steht schon Alles im Helmholtz.' This comes very near, in fact, to being a literally true description of this monumental work.

It is to be regretted that Professor v. Helmholtz was not able to give one more year to the revision of this book; his death occurred in 1894, and for a number of months before that time his attention had been so much absorbed in other branches of science—notably in meteorology—that he had practically abandoned doing any further work on the Optics. The last third of the book was brought out under the supervision of Professor König—of course, without changes from the original text. Professor König has also added a voluminous and carefully classified bibliography, covering some three hundred pages, and extending to the end of 1894; many of the investigations which have been incorporated into the body of the book are his work and that of his pupils. There is an editorial note of Professor König's on page 1008 which must not be overlooked, as it is of much importance. He points out that an error of calculation was afterwards detected in Helmholtz's determination of the fundamental colors by means of an extension of the psycho-physical law; that the master had regarded the investigation from the beginning rather as of the nature of a proof of the utility of the idea than as a means of arriving at definite results; and that he had come more and more to the conviction that the fundamental colors derived from their investigations by König and Dieterici not only met the conditions laid down by them, but were also consistent with a rigid upholding of the principle referred to.

It is also to be regretted that the revised volume had advanced so far that it was impossible to take account in it of the remarkable investigation by which Professor König has shown the coincidence between the relative absorption of light of different colors by the purple pigment of the rods and the relative luminosity of the spectrum as seen by the totally color-blind. v. Helmholtz had taken naturally the greatest interest in this investigation, and it would have constituted an important chapter of his book; this, together with the discovery by Hering of the identity of the vision of the totally color-blind with the night-vision of the normal eye may be regarded, doubtless, as the most important contributions made to psychological optics for many years.

If Hering will probably fare no better than v. Helmholtz in regard to the two main points on which they have been at variance, there is a matter of minor interest concerning which his treatment is very much to be preferred to that of v. Helmholtz; in giving a mathematical discussion of the horopter, Hering has had the happy idea of making use of the methods of modern projective geometry. These were evidently wonderfully well adapted to the question, and they enable him to avoid the extremely long and complicated investigation of the question which is given by v. Helmholtz.

C. LADD FRANKLIN.

PSYCHICAL RESEARCH.

A Further Record of Observations of Certain Phenomena of Trance. RICHARD HODGSON. Proceedings of Society for Psychical Research, Part XXXIII., Feb. 1898, Vol. XIII., pp. 284-583.

A continuation of the case of the test-medium, Mrs. Piper, already reported on in previous 'Proceedings.' The present account is based on the results of 500 more sittings, about 130 of which were with unnamed strangers introduced to Mrs. Piper, for the first time. The almost exclusive 'control' up to 1892 was a personality named Phinuit, concerning whose earthly identity no evidence has turned up. Since 1892, however, the principal control has, until a year ago, purported to be the spirit of G. P., a young literary man recently dead in New York. The most striking feature of the present report is the expressed opinion of Dr. Hodgson, that the communications of G. P., as well as of others, now seem to him more naturally explicable on the hypothesis of spirit-return than on any other hypothesis. This conversion to spiritism of so critical an investigator, until lately disinclined to any such conclusion, marks, of course, the passage of a 'critical point' in the history of the Society for Psychical Research, as well as in Dr. Hodgson's own career.

The phenomenon, briefly described, is as follows: The medium waits passively for the trance to come on, which it now does quietly, though formerly there was a good deal of respiratory disturbance and muscular twitching. 'Phinuit' used to communicate entirely by speech, but G. P. early manifested himself by seeking to write on a pad placed on the medium's head. He now writes on the table. 'Phinuit' may talk whilst the hand is writing on other subjects, often

under controls different from G. P., and purporting to be deceased friends of sitters. After two hours, more or less, the communications grow 'weak' and confused, and Mrs. Piper emerges from the trance, often with an expression of fear or distress, and usually with incoherent expressions on her lips, which Dr. Hodgson ascribes to her own subliminal consciousness, as distinguished from her consciousness under complete control. These intermediary and fragmentary expressions he considers to be also worthy of study.

The remarkable feature of the trances is the supernormal knowledge which the medium in a majority of cases displays of her sitter's private affairs. This knowledge is incoherent, fragmentary and, as a rule, of unimportant matters. The communications most convincing to those who received them could, out of deference to the natural dislike to publicity of sitters, not be printed at all, so that the evidence now offered to the reader is by no means full-strength.' Dr. Hodgson gives copious specimens of it, however, such as it is, in most of its varieties, including complete failures amongst the rest. It is intolerably tedious and incoherent reading; and one can but admire, along with the pertinacity of the reporter and his scrupulous accuracy, the manner in which his memory retains the threads of cross-connection among the parts of the system, and is able to bring points in one sitting to the illustration of points in another. Certainly there never before was such a conjunction of a good medium with a thorough investigator—and in this respect the report marks an epoch in our knowledge of trance-states.

Dr. Hodgson considers that the hypothesis of fraud cannot be seriously entertained. I agree with him absolutely. The medium has been under observation, much of the time under close observation, as to most of the conditions of her life, by a large number of persons, eager, many of them, to pounce upon any suspicious circumstance, for fifteen years. During that time *not only has there not been one single suspicious circumstance remarked, but not one suggestion has ever been made from any quarter* which might tend positively to explain how the medium, living the apparent life she leads, could possibly collect information about so many sitters by natural means. The 'scientist,' who is confident of 'fraud' here, must remember that in science as much as in common life an hypothesis must receive some positive specification and determination before it can be profitably discussed; and a fraud which is no assigned kind of fraud, but simply 'fraud' at large, fraud *in abstracto*, can hardly be regarded as a specially scientific explanation of specific concrete facts. In the concrete here, there is *no sign whatever*

that the medium when awake has any curiosity about persons, least of all about persons whom she has never met.

No, Mrs. Piper's trances are phenomena *sui generis*. Mr. Hodgson, admitting the element of supernormal knowledge in them as a fact, weighs against each other as two theories of its origin, first the supposition of telepathy from the sitters' and other living minds, and second, spirit-communication. He finds the latter theory to offer on the whole the least resistance, since a minute discussion of the points of success and failure shows that they fall into the simpler systematic order if we connect them with the departed personalities from which they profess to proceed. G. P., for instance (with one exception, which Mr. Hodgson explains), always recognized his old acquaintances (30 in number) when anonymously introduced as sitters, and rightly called them by name, but similarly recognized no one else. Obviously such selection round G. P. as a center would be less simply explicable were the medium tapping the consciousnesses of the sitters for their names, than were an independent personality with G. P.'s actual mundane memories a factor of the case. Again, the very confusion of many communicators, identified by sitters, and their inability to bring out more than a few rudimentary facts about themselves, points rather to a genuine spirit-presence obstructed in its means, than to telepathy from the sitters, whose minds, full of other facts relevant to the case, might apparently be drawn upon for them as easily as for those already given. In brief: "There are various selections of information given in connection with various communicators which are intelligible if regarded as made by the communicators themselves, but for which there is no satisfactory explanation to be found by referring them to Mrs. Piper's personality. With one class of *deceased* persons Mrs. Piper's supposed telepathic percipience fails; with another class it succeeds; and it fails and succeeds apparently in accordance with what we should expect from the minds of the deceased, and not in accordance with what we should expect from the minds of living persons acting upon Mrs. Piper's percipient personality" (p. 393). The case is a matter of balancing probabilities based on minute comparisons of detail, and Mr. Hodgson is far from ascribing certainty to the spiritistic conclusion which he adopts.

Mr. Hodgson fails to mention one feature of the case which may make for the spirit-hypothesis, and which will probably have struck other readers besides myself. No one can be conversant with his investigation of the Piper case without admiring the great grasp of memory of details which the investigator exhibits. And yet Mr. Hodgson's

memory is as nothing compared with Mrs. Piper's, who, with hundreds of sitters, many appearing only a few times, at years of interval, and conversing of inconceivably paltry personal details, seems never to fail to make connection again, or to take up the conversation just where it was left. Mr. Hodgson's memory covers fewer years, and taking and transcribing the notes of the sittings, as he does, and consulting and comparing the records *ad libitum*, he has a great advantage over Mrs. Piper. Yet he would be quite incapable of resuming conversation with former sitters as she does in her trance. Mrs. Piper's trance-memory, then, is no ordinary human memory; and we have to explain its singular perfection either as the natural endowment of her solitary subliminal self, or as a collection of distinct memory-systems, each with a communicating 'spirit,' as its vehicle. The choice obviously cannot be made off-hand.

If I may be allowed a personal expression of opinion at the end of this notice, I would say that the Piper phenomena are the most absolutely baffling thing I know. Of the various applicable hypotheses, each seems more unnatural than the rest. Any definitely known form of fraud seems out of the question; yet undoubtedly, could it be made probable, fraud would be by far the most satisfying explanation, since it would leave no further problems outstanding. The spirit-hypothesis exhibits a vacancy, triviality and incoherence of mind painful to think of as the state of the departed; and coupled therewithal a pretension to impress one, a disposition to 'fish' and face round, and disguise the essential hollowness, which are, if anything, more painful still. Mr. Hodgson has to resort to the theory that, although the communicants probably are spirits, they are in a semi-comatose or sleeping state while communicating, and only half aware of what is going on, while the habits of Mrs. Piper's neural organism largely supply the definite form of words, etc., in which the phenomenon is clothed. Then there is the theory that the 'subliminal' extension of Mrs. Piper's own mind masquerades in this way, and plays these fantastic tricks before high heaven, using its preternatural powers of cognition and memory for the basest of deceits. Many details make for this view, which also falls well into line with what we know of automatic writing and similar subliminal performances in the public at large. But what a ghastly and grotesque sort of appendage to one's personality is this, from any point of view: the humbugging and masquerading extra-marginal self is as great a paradox for psychology as the comatose spirits are for pneumatology. Finally, we may fall back on the notion of a sort of floating mind-stuff in the world, infra-human, yet possessed of

fragmentary gleams of superhuman cognition, unable to gather itself together except by taking advantage of the trance states of some existing human organism, and there enjoying a parasitic existence which it prolongs by making itself acceptable and plausible under the improvised name of a 'spirit control.' On any of these theories our 'classic' human life, as we may call it, seems to connect itself with an environment so 'romantic' as to baffle all one's habitual sense of teleology and moral meaning. And yet there seems no refuge for one really familiar with the Piper phenomenon (or, doubtless, with others that are similar) from admitting one or other, perhaps even all of these fantastic prolongations of mental life into the unknown.

The world is evidently more complex than we are accustomed to think it, the 'absolute world-ground,' in particular, being farther off (as Mr. F. C. S. Schiller has well pointed out) than it is the wont either of the usual empiricisms or of the usual idealisms to think it. This being the case, the 'scientific' sort of procedure is evidently Mr. Hodgson's, with his dogged and candid exploration of all the details of so exceptional a concrete instance; and not that of the critics who, refusing to come to any close quarters with the facts, survey them at long range and summarily dispose of them at a convenient distance by the abstract name of fraud.

WILLIAM JAMES.

Le Suicide: Étude de sociologie. E. DURKHEIM. Paris, Alcan, 1897. Pp. xii+462.

According to Professor Durkheim's definition, suicide is every case of death resulting directly or indirectly from a positive or negative act accomplished by the victim, with the knowledge of its producing just that result (p. 5). It is not an act of the individual, determined exclusively by individual factors. There are 'social' causes of suicide entirely distinct from those which concur in determining suicide in every particular case (p. 13). This is proved by the relative stability of the suicidal rate from one year to another in one and the same society, and by the great variableness of the suicidal rate from one society to another in one and the same period. Every society, says D., is predisposed to furnish a determined share of voluntary deaths (p. 15). This 'collective' tendency to suicide, which every social group shows in a very characteristic way, finds its numerical expression in the proportion of the absolute number of voluntary deaths to the population of every age and sex. This is what D. calls 'taux de la mortalité-suicide' (p. 10). The first book of D.'s essay deals with

the question whether such a collective tendency to suicide be the result of extra-social factors, like the organico-psychical predispositions (insanity, alcoholism, race, heredity) or the character of the physical environment (climate, seasons, temperature). The answer is decidedly negative. In book second D. discovers the 'social' causes of suicide. There are three types of suicide, (1) the 'egoistic,' (2) the 'altruistic,' (3) the 'anomic,' to which three orders of causes correspond, respectively, (1) the lack of social integration caused by relaxation of social ties, be they religious, family or political, (2) the absorption of the individual by the community to the point of destroying the consciousness of his own personality, (3) the sudden breaking down of the social control over individual desires, brought about by perturbations in the collective organization. These are, according to D., the 'social' causes of suicide; that is to say, the real motives of the propensity to suicide which statistics detect in every society. The suicidal rate (*taux des suicides*) cannot but be explained sociologically (p. 336). It is the moral constitution of the group which determines, at every moment, the share of voluntary deaths to be furnished (*ibid.*). The individual conditions—chiefly mental—can only explain why some yield to the 'courant suicidogène' and some do not; but cannot account either for the distinctive character or for the intensity of the 'courant' itself. All those who commit suicide may be said to be 'névropathes,' at large, but, as D. textually says, "*ce n'est pas parce qu'il y a tant de névropathes dans un groupe social qu'on compte annuellement tant de suicidés*" (p. 366).

For the psychologist, the interest of D.'s book lies mainly in the fact that it affords a positive proof of the impossibility of accounting for anything in social life without presupposing the working of a fundamental law of inter-cerebral action through which thought—the only 'social' matter—is imitatively transmitted from one individual to another. This law is, I might almost say, the window through which psychologists have been allowed to gaze upon the sociological field, and to detect some of its previously unknown peculiarities. What is now, somewhat inappropriately, being called the 'psychological' view of society is just the conception of society growing out of the assumption that social intercourse is, essentially and elementarily, the action of one brain upon another brain. This assumption is so fundamental for every attempt to interpret the process of social organization that we can give the 'social' causes of suicide whatever meaning we may choose; we can even admit D.'s 'constraint' view of the social phenomenon, and consider the 'courant suicidogène' as some-

thing extraneous to the individual, entirely independent, in its very nature, from any individual factor, and yet find ourselves, at the end, confronted with the question: How can these alleged 'social' causes, or, as D. puts it, the 'courants suicidogènes,' act upon the individual? The cause being a 'social' one, that is to say, in D.'s conception, something independent of individual factors, the phenomenon, on the contrary, taking place in the individual, we must know how the 'social' cause can reach the individual, how the 'collective' force can succeed in individualizing itself. What is, in other terms, the way of propagation of the 'courant suicidogène'? D. answers: by constraint. But this is mere tautology. How is this 'constraint' exerted? By what process is the individual penetrated, invested, dominated by the mysterious 'courant'? On this point D. is dumb, and it is this the great gap in his social theory which ignores how the social forces, *i. e.*, in terms less transcendental than D.'s, how the examples set in the social milieu are incorporated by the individual, how the institutions, which are the essentially 'social' thing, the most genuine and typical product of 'collective' activity, come into contact with the individual and exert a modifying influence upon his own personality. As I have remarked elsewhere (*Am. Journ. of Soc.*, January, 1898) these alleged 'social' causes of suicide appear to be nothing but *verbal* entities if we do not give a positive, precise and definite meaning to the vague formula of the action of society upon individuals, *i. e.*, if we do not identify it with the transmission of modes of thought and feeling from one individual to another through the imitative response to the inventive suggestiveness. Thus, while attempting to furnish the best evidence of the truthfulness of his own social theory—the 'constraint' theory—D. has shown, in an admirable although unintentional way, the soundness of the opposite view of the social phenomenon, as unfolded in the Tarde and Baldwin theory of imitation.

GUSTAVO TOSTI.

The Mathematical Psychology of Gratry and Boole. By MARY E. BOOLE. London, Swan, Sonnenschein & Co. 1897. Pp. 116.

This book is one of the many small non-technical works now appearing in the borderland of psychology, which start in with interesting if vague suggestions but fail to work them out into any useful concrete form. It aims to show that mathematical methods can be introduced into psychology, but ends by laying down no more than a very general canon of method which is derived from mathematics hardly more than from any other science. The author would carry

Boole's idea that reasoning, advance of knowledge, is a strictly mathematical process, into every department of life, and therefore mathematics is for her the universal 'Science of sane inspiration.' The method of advance in psychology must then be the same as that in mathematics. Now, the main feature of mathematical reasoning is what Mrs. Boole calls the 'Law of Sacrifice.' We devise a convenient fiction, to enable us to get what truth will be serviceable to us. To measure a curve we use the notion of indefinitely shortened straight lines, not really existing in the curve. In constructing a figure we draw many construction-lines, which we afterwards erase. The fiction when used is thrown away, sacrificed, and only the result is kept. Just so, in daily life, you have one opinion, I another; each is partial and erroneous by itself, and the truth combines the good of all. And the best way to show a man his errors is, first to admit the truths they contain. In all this there is scarcely any psychology, still less anything novel. We are not told why the 'Law of Sacrifice,' which in practice we all obey more or less, is best called mathematical. Still there are hints here and there which save the book from being only a general affirmation of the metaphysical value of a whole, and give it a leaning towards psychology. The conventions we find useful in mathematics are those dictated by our own nature. We count by the ten fingers of the hand; we represent inconceivably great numbers by such signs as can be easily remembered. There is thus an element of psychology in our mathematics, and indeed as one begins to read the book he is led to hope that he will be told why the points of view suggested by our own psychological natures are so fertile in yielding valuable results in mathematics. One hopes for an answer to such questions as: What is there about the quantitative point of view that should make it so useful? What does the conception of quantity really mean? Only by answering such questions can we tell how far the conception of quantity, or any mathematical conception, can be applied to psychical contents. In short, what is needed is analysis of those points of view which psychology furnishes to mathematics. If we were perfectly sure what we meant by greater and less we might know in what sense we could say one sensation had *more* of a certain quality than another. But without detailed analysis there is no hope of understanding the relations between psychology and mathematics. If psychical contents cannot be measured, it is not the 'Law of Sacrifice' that will prove it.

The merit of the book lies rather in what it suggests than in what it accomplishes. It works out nothing new, but suggests by its very

failure to do so that a different line of inquiry must be pursued by those who would examine the relations between mathematics and psychology. The real meaning of the mathematical conceptions has only recently begun to be analyzed, but the tendency toward this analysis is a growing one; and when we have general, non-technical works attempting to extend it into a psychology for every-day usage, it is evidence that it has already got considerable momentum. But, to accept analogies for identities, while it cannot hurt one's mathematics, may injure his psychology, for it makes him blind to obvious facts, and kills the empirical spirit. An exact analysis is the only safeguard against unsubstantiated claims for mathematics on the one hand, and unfounded prejudice against it on the other.

The latter part of the book is designed to be of practical use in finding truths, convincing one's opponents, etc. The general rule is that the truth is never seen from an isolated point of view, but includes many partial views in an organic whole. This, however, has no special connection with psychology or mathematics, but assumes a strongly religious coloring; and at the end we are told that "Unity is the property of the Infinite, the Absolute, the Eternal; Dividedness is the property of the finite, the phenomenal, the transitory."

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La Fatigue intellectuelle. A. BINET and V. HENRI. Paris,
Schleicher Frères. 1898. Pp. 338.

We have here not only another book from the prolific laboratory of the Sorbonne, but also the first volume of a *Bibliothèque de pédagogie et de Psychologie*. According to the *Psychological Index*, M. Binet made seventeen publications during the year 1897, leading, *longo intervallo*, Professor Baldwin, who stands next in the list with eleven. Easy writing does not, however, always make hard reading, and M. Binet has the talent for clear exposition that distinguishes his nation. This book on mental fatigue, the one in preparation on the training of the memory and the others to follow will perform an important service both for psychology and for pedagogy. Extravagant claims have, on the one hand, been made regarding the practical value of experimental psychology to the teacher, while, on the other hand, the validity of these claims has been altogether denied. A book such as this one on mental fatigue shows pretty clearly what psychology has done and what it hopes to do. It must be acknowledged that the promise is greater than the performance, yet the latter is not altogether insignificant.

The book opens with a chapter on a discussion regarding the overburdening of school children, held before the Paris Academy of Medicine some ten years ago, inserted by the authors to show the futility of mere opinion as compared with the value of the experimental research since accomplished. The second chapter is on the definition of mental work, and the rest of the book is divided into two parts, one on the physiological effects and the other on the psychological effects of mental work.

In the first part seven chapters treat, respectively, the influence of mental work on the heart, on the capillary circulation, on the blood-pressure, on the production of heat, on respiration, on muscular energy and on metabolism. It may be remarked that, while the title of the book is 'Mental Fatigue,' the parts are headed 'Mental Work,' and the latter description is the more accurate. With the exception of the chapters on muscular energy, the question of fatigue occupies a subordinate place in this section, and the applications to pedagogy are rather in the description of methods that may be used than in any results hitherto obtained. But if the section is of only limited interest to the teacher, it is of decided value to the psychologist, being the best existing summary of experimental work on certain of the relations of mental to bodily change. The subject has been worked over more especially by Italian and French students, and scarcely sufficient attention is given to it in the *Physiologische Psychologie*, Hermann's *Handbuch* and other German compendiums. For example, it is probable that many lecturers on physiology and psychology state automatically that a decrease in the volume of the arm accompanies thought, and that this means that the blood goes from the arm to the brain, not knowing that Mosso has himself changed his views on the subject, and now believes in the independence of the blood supply to the different-organs. Again, Mosso's balance is an experimental method often quoted, and MM. Binet and Henri, while not challenging its validity, at all events pass it over with such slight notice that suspicions may be awakened in regard to it.

The first section, as is natural, pays special attention to work done in the laboratory of the Sorbonne, particularly that on capillary circulation. The authors admit that the observations are as yet too few "pour qu'on sache le parti que la pédagogie pourrait en tirer," and they do not conceal the technical difficulties of the method. In giving traces of the capillary pulse of children (p. 75) to show individual differences, they, however, seem to neglect the fact that the differences may be due—I should suppose are chiefly due—to the method of record-

ing. All these methods suffer greatly from their lack of quantitative definiteness, a slight alteration in the instruments, or in their adjustment, altering entirely the character of the curves. The authors hold that the fact that the vasomotor changes follow the stimulus after an interval of about two seconds disproves the James-Lange theory of the emotions. It seems to me, however, that this fact rather supports the theory, as it is a matter of experience that the emotions do not immediately follow the external cause like a reflex movement, but gradually develop and increase in intensity.

The second part of the book discusses the psychological effects of mental work. Here the authors are really concerned with fatigue, and in part with the fatigue of school children, so the subject matter is more germane to the title of the book and the name of the series than the first part. But the field is rather limited, only a dozen researches being recorded. Thus, 33 pages are given to an abstract of Oehrns thesis, which is itself only about twice this length, and is sufficiently accessible as reprinted in Kraepelin's *Arbeiten*. The other researches carried out by Kraepelin or under his direction are also reported at length, and are the only laboratory researches quoted on mental fatigue. They are, perhaps, the only ones of importance, but a collection of scattered work might have been useful. Thus, I myself have tested fatigue by making 1,950 reactions in succession, which is an undertaking not easy to repeat. After the review of Kraepelin's work we have four chapters on experiments made in the schools. These review the method of testing fatigue by dictations used by Sikorsky, Höffner and Friedrich, the method of performing arithmetical calculations used by Burgerstein, Laser, Holmes and Richter, the method of Ebbinghaus, and the decrease of tactile sensibility claimed by Griessbach. The authors seem rather unduly critical in their review of Ebbinghaus's work, and uncritical in the case of Griessbach. His results, showing greatly enlarged sensation-areas in the course of school work, seem on their face unlikely; I have myself entirely failed to confirm them. The authors conclude their book by expressing the hope that the French government will become convinced "that no pedagogical problems can be settled by discussion and oratory, and will favor to the utmost researches in experimental psychology carried out in the schools."

J. McKEEN CATTELL.

Studies from the Yale Psychological Laboratory. Edited by E. W. SCRIPTURE, Ph.D. Vol. IV., pp. 141. 1896.

This volume of studies gives the results of the fourth (1896) year's work at Yale. It contains accounts of one research and of about a dozen introductory studies, to which the editor adds some notes on new apparatus and instructions for about one-half the laboratory exercises in his introductory experimental course.

Dr. Seashore reports the most extended investigation of the year, 'The Influence of the Rate of Change upon the Perception of Differences in Pressure and Weight.' With ingenious arrangements of apparatus, using various rates from instantaneous to the slowest that allowed a difference to be perceived, he found for pressure a threshold at first (instantaneous; can this be impact?) very small—only 7%, then as the rate decreased to 6.6 grams per second, the threshold rose very rapidly, and again fell gradually to the point where the rate was so slow as to give a barely perceptible increase. A curve of a similar character was found for the perception of weight. It is unfortunate that the author refers only by title to previous work in the same direction, without giving any indication of its relation to his own. Particularly is it to be regretted that he did not attempt to reconcile his results with those of Stratton (*Philos. Studien*, XII.).

A second article by Dr. Seashore, 'Weber's Law in Illusion,' has already been described in this REVIEW (Sept. 1897, IV., 522) and needs no further mention.

The remaining experimental articles of the volume are short, introductory studies on certain topics in reaction-time and in the force of movement. The most complete of these is a paper by Dr. A. G. Nadler, attempting to find the simple and complex reaction-time differences in normal and abnormal conditions of the nervous system. The abnormal cases chosen were local and multiple neuritis, locomotor ataxia, hysteria and alcoholism. The editor summarizes the results as follows : "Alcoholism shortens the simple reaction-time, hysteria leaves it unchanged, and local neuritis, multiple neuritis and locomotor ataxia lengthen it. Local neuritis slightly lengthens the additional mental processes involved in complex reaction-time, alcoholism lengthens them considerably, while locomotor ataxia, multiple neuritis and hysteria double and triple the normal time." The material does not seem to be quite homogeneous as to age, sex, etc., and any conclusions drawn can be only tentative. Moreover, many differences which Dr. Scripture considers typical will be found to fall well within the limits of the mean variation. It should be noted,

too, that the three cases of alcoholism tested before and after treatment show (seemingly in contradiction to Dr. Scripture's conclusion) a shortening of the time after treatment, when they may be supposed to be more nearly normal.

Other experiments are said to "show an almost universal quickening of both the simple and complex times under the stimulus of the electric current" through the head. The almost universal is found to be eighteen cases in twenty-four, with eight of these within the ordinary limits of variability. Another series shows that the more complex the adjustments required for perceiving the stimulus for light reactions, the greater the increase of fatigue in reaction-time. "There is least fatigue when only an effort of attention is involved, more when the act of accommodation is added, and still more when the act of convergence is also added." The fatigue incident to the ordinary day's work caused an average lengthening of simple reaction by 19σ and of complex by 24σ (three subjects). When the reacting finger raised a weight (the attention was doubtless on the finger throughout the experiment) before and at the time of reaction, the time was shortened (sound by 15σ , light by 14σ). When the weight acted on the finger only during the reacting movement the results varied. When the subject reacted with a slight or strong effort without a weight, the latter showed a shortening of $15-31\sigma$. The value of these last six studies would have been much greater if the same constant conditions had been used in each series. Then the results would have been comparable.

The reaction (reflex ?) times of two cats to electric stimuli were found to be, respectively, fore-foot, 96σ , 41σ ; hind-foot, 116σ , 62σ ; lips, 61σ , 62σ .

The 'Researches on Voluntary Effort' can only be mentioned. They are (1) mental scale of effort for thumb and forefinger, (2) scale for the forearm and hand, and (3) fatigue in voluntary movements.

Under the heading 'New Apparatus and Methods,' Dr. Scripture describes a new adjusting clamp and an improvement on his multiple key, and tells how the projection lantern may be used for purposes of psychological demonstration, particularly in color and stereoscopic vision, and in chronographic experiments. Psychologists will also be greatly indebted to the editor for his account of a clean and generally satisfactory method (the 'so-called' lamp battery) of producing electric current of low potential where the electric lighting supply is available. For the benefit of those not so familiar with electric science,

the principle might have been explained. The reviewer cannot see that the principle is 'explained by describing the method of construction.' Several corrections should be made in Tables I. and II. to make the results of value. 'Trade Names' should be supplemented by 'measurement.' Had this been we would not see that m , $n = o$, but that Am , An and Ao give three different results. Cf., also further combinations of m , n and o , and of q and u . In the discussion of the relative value of the lamp batteries and that of a motor transformer, the author does not seem to understand clearly the advantages of the latter. He says: "It (a transformer) is quite costly and also inconveniently heavy. A laboratory of any size can hardly do with an equipment of less than ten batteries; *such a set of motor dynamos would be quite beyond the reach of most institutions.* A larger motor dynamo might be used to distribute a low voltage current throughout the laboratory." It would be, indeed, absurd to use a separate transformer for each piece of apparatus or even for each room, a one-half H. P. machine being large enough to supply the present needs of any psychological laboratory. The cost, too, is not so enormous, not so much as many pieces of apparatus of lesser general utility, considered essential by laboratory directors. The running cost will be found to be less for the motor dynamo than for the lamp battery.

By far the most interesting part of the book is the final article by the editor on 'An Elementary Course in Psychological Measurements.' The full course is not given, thirteen experiments being omitted. The remaining seventeen can be grouped under the categories of energy, time and space (see *The New Psychology*, 1897), although no order seems to have been followed by the author here. The fifty-one pages will be a very welcome addition to the other laboratory manual that we now possess, and it is to be hoped that Dr. Scripture will soon publish in book form the directions for his entire course. A critique would then be more appropriate.

In comparison with its predecessors this volume is less satisfactory in the details of experiments, but it has gained considerably in its diversity of subjects and its suggestiveness. The wonderful diligence and enthusiasm of the director, officers and students of the New Haven laboratory doubtless will be a stimulus to many other workers who will look forward to the early publication of the work for 1897.

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Problems in the Psychology of Reading. J. O. QUANTZ. Monograph Supplement, No. 5, to PSYCHOLOGICAL REVIEW, December, 1897.

This is an attempt to trace experimentally some of the factors which determine one's rate of reading and the amount of information obtained. The study does not deal with methods of learning to read, and only very slightly with external conditions, but investigates some of the processes involved in the ordinary reading of adults, and determines a few of the mental characteristics which are favorable to the most rapid gaining and surest retaining of thought from the printed page.

The tests of visual perception, by means of an exposure apparatus, show that words in construction are recognized and named more quickly than disconnected words, these isolated words more quickly than colors, and colors than simple geometrical forms. The number of each read per second (averaging the one-half and one-second exposures) is 9.4, 6.2, 4.6 and 4.2 respectively, the differences being due mainly to the strength of the association between the perception of the object (form, color, word) and its name. The correlation of reading rate—the rate being averaged from maximum and normal speed of silent reading under ordinary conditions—with quickness of visual perception, shows this latter, with its large physiological element, to be an important factor in the determination of speed of reading. Other influences contributing to the rapidity of reading, in the probable order of their importance, are: practice, as determined by amount of reading from childhood onward; power of mental concentration, measured by ability to resist distracting stimuli; general mental alertness, estimated by rapidity of original composition, which was believed to be a more adequate test than arithmetical operations, 'finding-time,' or complex reaction-times; scholarly ability, as decided by college records.

The eye-minded student is more rapid in his reading than the ear-minded. The comparison between eye and ear was made in three ways: (1) By testing the visual and auditory memory span; that is, the limit of power to repeat, with absolute correctness in matter and arrangement, lists of words read or heard once—beginning with groups of four and increasing to the limit of memory. Here the use of eye and ear together, the words being read *aloud by the subject*, is little advantage over either separately, where the words are read *to him* or *silently by him*. In fact, in the case of decided 'visionaires' or 'auditeires' the simultaneous use of a second sensory avenue may be an

actual hindrance, by interfering with the concentration of attention upon the sensations coming through the first. (2) Eye and ear were compared by their ability to detect differences between two variant readings of the same passage, the changes consisting in the substitution of one word for another, usually synonymous, the insertion or the omission of a word, and the inversion of the order of phrases. Of these different changes insertions are by much the most easily recognized; then follow in order the inversion of phrases, the substitution of words, and the omission of words, which is the most difficult of detection. In this test the auditory process showed a great superiority over the visual. (3) The final comparison between eye and ear was made by reproducing the thoughts of two selections, one of which was read to the subject, the other read silently by him *at the same time*, the subject making an effort to recall as much as possible of each and not giving attention exclusively to either. As determined by this test "the degree in which the rapid readers excel the slow in eye-mindedness can best be understood by a comparison of the extreme classes. The 'very slow' readers (3.9 words per second) reproduce 89.1% as much of the visual selection as of the auditory, while the 'very rapid' readers (7.3 words per second) are able to recall 123.2 of visual for every 100 of auditory; that is, the ratio of reading rates between slowest and fastest readers is 3.9 to 7.3 (1:1.87), while the ratio of the visual tendency to the auditory is 89.1 to 123.2 (1:1.38). On the principle of correlations this result shows eye-mindedness to be a rather strong factor in the determination of reading rates." The greater rapidity is not gained at the sacrifice of exactness or of intelligence. "A comparison between the ten most rapid readers and the ten slowest shows that the rapid readers remember more of the original thoughts, and that the character of their reproduction is much higher, both generally and with reference to expression and to logical content. In the auditory tests the ratio between slow and rapid readers is 14.8% to 20.7% in the *number* of thoughts recalled. In *quality* the percentages are 47.8 for slow readers and 60.3 for fast. The same comparison in the visual tests results as follows: Percentage of thoughts reproduced by slow readers, 14.9; by rapid, 24.4. Quality: slow, 48%; rapid, 73.3%. The difference in favor of the 'rapids' is consequently much greater than in the auditory tests, indicating again that rapid readers are, as a rule, of the visual type." The advantage of rapidity of reading is further shown by a comparison in all the auditory-visual tests. In memory span the rapid reader is superior to the slow by 26.7%; in the recognition of variations in the two read-

ings the percentage is 43.7; in the simultaneous processes, 40.5. "The superiority of the rapid reader is also shown by the fact that his memory of the substance of his reading is more exact than that of the slow reader. He introduces only two-thirds as many thoughts not found in the original selections." In reading aloud, moreover, if it is to be intelligent and intelligible, words must be perceived some distance in advance of those which the voice is uttering. The rapid reader has the greatest interval between eye and voice, so that it follows here again, as in silent reading, that rapidity is an advantage.

Motor-mindedness, which is usually manifested in lip-movement in silent reading, is found to be a serious hindrance to rapidity of reading. The ten most decided lip-movers read 4.1 words per second, which places them between the classes 'slow' and 'very slow,' and nearer to the latter; while the ten who show least movement of lips read 5.6 words per second—very close to an average 'rapid.' Lip-movement and total amount of reading are in inverse ratio; that is to say, practice in reading continues indefinitely to decrease lip-movement. From the conclusions already reached as to the relation between rapidity and intelligence of reading, it can be inferred that the quality of intellectual work is lower in persons of decided motor-mindedness. This conclusion is justified also by a direct comparison. Lip-movement in silent reading is believed to be not an acquired habit, but a reflex act the tendency to which is inherited. As one's methods of study, and of intellectual work in general, are improved, lip-movement, with its retarding influence and its unnecessary expenditure of energy, is gradually outgrown.

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Experimentelle Untersuchungen über das Zusammenwirken des akustisch-motorischen und des visuellen Gedächtnisses. J. COHN. Zeitschrift für Psychologie und Physiologie der Sinnesorgane, Bd. XV., Heft 3, p. 161, Sept. 14, 1897.

The author describes experiments begun in the summer of 1894, in conjunction with Dr. V. Henri, who did not, however, aid in completing them. C. exposed to the view of his several subjects, for a sufficient time to have them read through twice, cards containing 12 letters (3 horizontal rows of 4 letters each). The reading, in the cases of 10 of his subjects, the study of which forms the greater part of

the article, was done in 3 ways, changed about regularly so as to equalize the effects of practice, sequence and fatigue. They were read by the subject (1) aloud, or (2) to himself with lips firmly closed and the tongue held close to the roof of the mouth, or (3) he read them to himself, at the same time pronouncing a vowel. The card was then taken away and he counted up to 20 (for 10 seconds) which usually, though not always, destroyed his memory image of the letters seen. Then he was to recall the whole card, putting, if possible, the right letters in the right places. The subjects of this experiment were 8 men, a boy of 10½ and a young woman. All the men remembered less from the second manner of reading (where no utterance was permitted) than in the first (where they read aloud the numbers they saw), and less from the third manner (where the subject, reading to himself, spoke aloud a vowel) than in the second. This would not indicate any variety of types, but with 6 of the 8 men the differences were greater in the three kinds of experiments than with the two others, and, of these six, 4 were accordingly judged to belong to the auditory-motor type.

The number of mistakes was tabulated, in addition to the number of correct answers (which latter consisted in remembering the right letter in the right place). The mistakes of putting a letter in the wrong place were considered as transpositions of groups, of horizontal rows, of letters in the same row, of letters of the same length above the line (b, d, etc.) or below the line (g, j, etc.), of letters sounding alike (be, pe, etc., el, em, etc.).

The tables are followed by observations of the individual subjects as to how they remembered, their statements sometimes confirming, and sometimes not, the type-characterization obtained by the figures. C. thus experimentally proves that a memory depending upon auditory-motor images is more confused by disturbances of an auditory-motor nature than is a memory relying chiefly upon the visual element, and that where the auditory-motor memory is interfered with by external stimuli of an auditory-motor nature, the visual memory will usually come to its aid.

These experiments also corroborate in a general way the results obtained by Münsterberg and Bigham (in the REVIEW for January and September, 1894), viz.: that the memory works better if the words to be remembered are both seen and heard by the subject, and that an auditory interruption affects the auditory memory more than a visual, and *vice versa*. They also confirm the results of Müller and Schumann (*Zeitschr. für Psychol.*, November, 1893), that in

rhythmic memory, every articulation of the rhythm is more closely associated with the accent than the latter is with its absolute position in the rhythm. The present writer has repeated C.'s experiments upon himself to some extent, and finds that his memory of the letters is in its recall solely auditory—whatever the manner of reading or however the interim (*Zwischenzeit*) is filled in, for he has persistently failed to visualize letters of the alphabet or to detect while reading to himself the slightest sensation in the throat or lips, or even motor imagery. Half of his correct answers came from his making words (in some cases nonsense words) out of the consonants, which words recurred to him as *heard* words in all cases. The letters read to himself while saying '*Ah*' were almost as well remembered as those read aloud, and both better than those read to himself with mouth closed.

Di alcune associazioni verbali. G. GUICCIARDI and G. C. FERRARI.
(Reggio.) Rivista Sperimentale di Freniatria, etc. Vol. XXIII., No. III., October, 1897. Pp. 649-81.

Guicciardi and Ferrari examined 54 persons of both sexes, aged from 6 to 67 years, to find out from their verbal associations to what 'type' (visual, auditory, motor, etc.) they belonged. Five suffixes were put one after another on the top of a card, and the subject was given ten minutes in which to write as many words having those suffixes as he could think of. It was thus a test of a person's rhyming abilities. The number of words written each minute was recorded in every case. After each experiment the subject was asked how he thought of the words he had written. The average number of rhymes was: men, 30; women, 27. (There are in the Italian rhyming dictionary 115 rhymes collected for the five suffixes.) The persons over 30 years of age are shown to have the greatest facility for rhyming. The largest number of rhymes thought of by one person was 66 and the smallest nine. The men, as a rule, wrote all the words of one suffix they could think of and then passed on to the next, and the women went from one to the other putting down a rhyme under each until their 'stock' of words was exhausted. The women who followed the masculine order of writing were known in other ways to be of a higher intellectual development than the other women, and the men who wrote in the feminine order were known to be more effeminate than the other men. One was hysterical.

The diagnosis of 'types' of mental imagery is not satisfactory in this article, however. First, the associations collected from all the subjects were classed as follows: Associations (1) by assonance, (2)

visual, (3) *ad literam*, (4) ideative, (5) motor. If in any subject association by assonance predominated, he was characterized as auditory; if visual association predominated, he was a visualist, etc. A physician was observed to be tracing the outline of the letters of the suffix with his finger; therefore he was primarily motor in his imagery. But we are told that the larger part of the associations of the first three minutes were those by assonance, and that the visual associations were the least in number for the whole time.

The German article confines itself to the consideration of the two narrow applications of its results, *i. e.*, the coöperation of the visual and auditory-motor memory, and the effect of rhythm in the latter; while the Italian article generalizes about sensory, emotional and intellectual types and the principle of least resistance in mental life. Neither article, however, exhibits a thoroughly satisfactory method of even estimating the relation between the various types of mental imagery as exhibited in one individual or between the manifestations of the same type in different individuals.

WILFRID LAY.

The Fluctuation of Attention. JOHN PERHAM HYLAN. PSYCHOLOGICAL REVIEW, Monograph Supplement, No. 2, Vol. II.

The *motif* of this study is the explanation of the changes and inconstancies which people experience in their attitudes towards other persons, occupations, amusements, etc., which are not explained by the objective changes in things themselves. Thus, a person suddenly has a feeling of revulsion for his occupation, or a heightened appreciation for another's society, without any observable change taking place in either. The same phenomena obviously occur when school children tire of a prolonged study, or when any of us return with renewed enthusiasm to a discarded piece of work. Evidently the attention changes its direction in these cases; hence, the title.

The parallel physiological phenomena are illustrated by a cut showing the result of ergograph experiments adapted for the purpose. These show that the amount of work done before fatigue makes movement impossible depends upon the supply and rate of supply of nervous energy—the same energy used in all mental activity. Then follows an experimental section in which seven different methods are employed for the solution of the problem.

The first experiment aims to produce fatigue of the attention, and to register the consequent decrease in the quantity and quality of work done by means of a revolving tape arrangement upon which

are letters of the alphabet which the subject reads at his most rapid rate. A somewhat irregular periodicity was found to occur of times in which the subject was obliged to omit groups of letters on account of fatigue. These times increased in length and frequency as the experiment went on. The second experiment allows a more careful analysis of the effects of fatigue. A series of forty-five columns of figures is arranged on sheets of paper and the subject required to add through the sheets as rapidly as possible. The experimenter takes the sum given for each column, and also the time needed for adding it. The sheets are added through fifteen times on as many days, and a somewhat elaborate method used for eliminating errors in the results of the experiment. Very marked changes in the rate of adding were shown to follow each other in rapid succession, especially during the first times when the feeling of fatigue was strong. In the third experiment nonsense syllables were learned in place of adding, but, as the syllables could not be made uniformly difficult to learn, changes in the rate of learning could not be referred exclusively to subjective conditions. The fourth experiment was suggested by the chance trial of an alternating electric current used as a distraction in the first experiment. It was used with the addition sheets, and the rate of adding compared with that under normal conditions. During the first trials there was a marked acceleration in the adding, but later the rate decreased. A similar experiment was tried with a music box playing a round of ten popular airs. The curves representing these two effects upon adding show a relative retarding influence of the music. The fifth experiment was planned to test the degree of independence in the mental functioning for different objects of attention. A series of ten simple diagrams and pictures were shown in succession to the subject, who looked at each as long as his natural inclination allowed, the time being registered for each view. A fairly constant relative period was found to be given to each view on the successive trials. It was also found that an unnatural prolongation of the attention upon a view on one day would often be followed on the next by a revulsion of feeling. The sixth experiment consisted of adding the addition sheets already mentioned twice in succession on alternate days, and the time for adding compared with that on the other days when the adding was preceded by learning nonsense syllables. In the second case the time was uniformly shorter. Here, too, the adding had far less mistakes. The last number in this programme consists of individual experiences of fluctuation in which the duration and nature of each attitude towards the object of attention was recorded.

The last section of the monograph consists of a theoretical application of the data of these experiments, together with current neurological theories, in a discussion of the nature and definition of attention, and its possible psycho-physical basis. The present writer will not descant upon the success or lack of success of this paper, aiming, as it does, to be hardly more than an outline study. I believe, however, that it suggests the possibility of adapting a broader range of psychological problems to laboratory methods than has hitherto been done, and also that in doing this much more exhaustive methods of technique must be used.

THE AUTHOR.

Methodologische Beiträge zu psychophysischen Messungen. DR.
ARTHUR WRESCHNER. Schriften der Gesellschaft f. psycholog.
Forschung. III. Samml., Heft 11. Leipzig, 1898. 238 pages,
7 marks.

This investigation of the accuracy of judgments in lifting weights begins with experiments in which weights attached to the wrist were lifted by a movement at the elbow joint.

The question is proposed: Can we properly consider the judgments 'smaller,' 'equal' and 'larger,' as three separate, independent forms of judgment with definite limits between them? The results show that if the different weights be represented on the axis of X to each side of the standard and if three sets of ordinates be erected proportional to the frequency of each kind of judgment for each weight, we obtain three overlapping curves whose maxima are at different points. The maximum for the judgment 'equal' is over the value for the standard, that for 'greater' is beyond the standard, and that for 'smaller' below it. The important fact established by these experiments is that the curve of frequency so closely resembles the usual exponential law assumed for statistics and measurements that we can hereafter with small error use such a law for discussion. Closer inspection, however, shows that these curves are slightly asymmetrical in the same sense as most statistical curves, namely, with an excess of large values; this tells in favor of the use of the median. These experiments afford an excellent occasion for calculating the results according to the scheme developed by Bruns (Phil. Stud., 1893, IX., 1); it is to be deeply regretted that the author has missed the opportunity. The author carefully discusses his results in respect to the error of sequence in its various factors, to practice, etc. In regard to Weber's law he finds that the mental processes involved in judging are different for small

weights, for moderate weights and for heavy weights; under these circumstances the law has only approximate validity. The investigation shows the marks of the careful methods employed in Professor Ebbinghaus's laboratory, in which the experiments were performed.

E. W. SCRIPTURE.

Ueber die Raumwahrnehmungen des Tastsinnes. Ein Beitrag zur experimentellen Psychologie von DR. VICTOR HENRI. Reuther & Reichard, Berlin. 1898.

In this monograph Dr. Henri gives us a critical examination of the present condition of research, experimental and theoretical, on the problems of the spacial perception of touch. All of the important work of others is recounted and many investigations of the author are described. But the aim of the treatise is by no means that of an objective account of the *status quo* of investigation on this subject. The treatment, while it endeavors to cover the whole ground, is dominantly critical.

The monograph is divided into two main parts—that treating of the facts and that treating of the theories. The former is opened by an important discussion on the nature of the threshold for two or more points to be distinguished through the sense of touch. Here the author concludes that the nature of this threshold has heretofore been generally misapprehended, that the sensations of one point pass gradually over to those of two through more or less distinct stages, and that the best method of experimenting on this subject is to have the reagent describe all of his sensations as completely as possible. These conclusions are then substantiated by means of a long series of researches on the threshold for two points conducted by the writer and Dr. G. Tawney.

From this the author passes on to discuss other thresholds, the 'Vexirfehler' and the experiment of Aristotle. Dr. Henri's researches on the last, which have already appeared in *L'Année psychologique*, form one of the most valuable and interesting portions of the work. The result is stated as follows:

"When two fingers are crossed and each is touched with a point, two points are perceived, the distance between which seems greater the smaller it actually is. Moreover, a reversal occurs so that the point to the left appears to the right and vice versa."

This much is contained in the first chapter. The second and third chapters deal respectively with the localization of impressions of touch and with a physiological and pathological treatment of the general

questions of the first part. Some of the work of Dr. Henri on visual localization with the use of a model is especially interesting. This is contained in chapter 2.

The second part is taken up chiefly with a presentation and examination of the theories of the origin of the spacial element in touch. After demolishing all other doctrines the author concludes by stating a possible hypothesis never yet presented, according to which the spacial moment in touch impression is neither 'innate nor original,' "but arises gradually in the course of development, although it is by no means a synthesis or 'chemical union' of non-spacial elements" (*sic*). A rather vain effort is made to keep the reader in the dark as to whether Dr. Henri himself holds this view or not.

The work is concluded with a general 'biologisch-psychologische Skizze' of the main question.

In a judgment on the value of the work the part relating to the experimental investigations must be considered apart from that concerning the theories. The former is thoroughgoing and careful in its references to the work of others, valuable in regard to the researches of the author himself, and in general satisfactory. But in the latter, which is, fortunately, by far the lesser in respect to size and importance, Dr. Henri seems to have gone somewhat beyond his natural line of work. The expositions of the theories, it is true, are clear and faithful, but the critical element is far from satisfying. The conditions and limitations of the solution of the genetic problem seem not to have been fully thought out.

PRINCETON.

F. KENNEDY.

Hypnotism and its Application to Practical Medicine. O. G. WETTERSTRAND, M.D., Member of the Society of Swedish Physicians at Stockholm, etc. Authorized translation by H. G. PETERSEN, M.D. Together with Medical Letters on Hypno-Suggestion, etc., by H. G. PETERSEN, M.D. G. P. Putnam's Sons, 1897. Pp. xvii + 166.

This brief treatise first appeared in Swedish, was almost immediately translated and published (1890) in German, appeared in Russian in 1893, and has won a deserved reputation. Dr. Wetterstrand has no theories to offer. He had had at the time of writing three and a half years' experience, had hypnotized 3,148 individuals, producing the hypnotic state about 60,000 times, and gives brief accounts of the results in 128 cases. In making the selection he has tried to give, as impartially as possible, both successful attempts and failures. The

diseases treated are grouped under thirty-one heads and cover a wide range. Dr. Wetterstrand's results agree with those of the School of Nancy. He has found suggestion no panacea, to be sure, but a valuable therapeutic agent, most successful in treating functional nervous diseases and functional psychic neuroses, but often useful even in organic diseases, either in relieving symptoms or in directly affecting the lesions. One of Dr. Petersen's essays contains an interesting account of Bernheim's personality and methods.

WM. ROMAINE NEWBOLD.

UNIVERSITY OF PENNSYLVANIA.

L'Attenzione e i suoi disturbi. By SANTE DE SANCTIS. Rome, Tip. dell' Unione Coop. Edit., 1896. Pp. 46. (Rep. fr. Atti d. Soc. Rom. di Antrop., Vol. IV.)

Dr. De Sanctis contrasts the incompleteness of the work on the pathology of attention with the prominence that has been given to the pathology of memory. After discussing the classifications of diseases of the attention proposed by Ribot, Emminghaus, Ziehen, Morselli and others, and the psychological theories of the attention held by Wundt, Carpenter, Meynert, etc., he rejects them all in favor of a new scheme. Instead of involuntary and voluntary attention he adopts the division into natural and conative, which corresponds nearly, though not exactly. The former admits of investigation only by the method of observation; the latter may be studied by experiment also.

Both natural and conative attention are of two distinct forms, which the author calls *introspective* and *extraspective*. For pathological purposes attention must be considered from the two standpoints of fixation and distribution. Considered according to their order of development, the power to fixate the attention upon a single object (accommodation or concentration) is to be regarded as a lower type than its distribution over a number of objects. Disorders of attention, either of concentration or of distribution, belong to three classes, which the author calls *aprosesses*, *hypoprosesses* and *hyperprosesses*. Some of these forms occur in normal life, as in the case of abstraction (undue concentration) and distraction (undue distribution). The aprosessive type, however, is strictly pathological, and is found only in idiots, etc.

The author's complete classification of the disorders of attention is as follows: I. Disorders of fixation, (1) by deficiency, = anaprosess and hypoprosess of fixation, (2) by excess, = hyperprosess of fixation. II. Disorders of distribution, (1) by deficiency, = restriction of the field of attention, or anaprosess and hyperprosess of distribution, (2)

by excess, = hyperprosess of distribution. III. Qualitative disorders of the attention, = paraprocess. This last class contains two distinct groups of phenomena, due to the entrance of conative attention into an act begun by, and properly belonging to, the natural attention. It may result, (1) in the repetition of some part of the act, such as the emphatic iteration of a phrase or word; or (2) in obstructing the normal flow of attention in such a degree as to prevent the completion of the act begun.

HOWARD C. WARREN.

PRINCETON.

Psychological Corollaries of Modern Neurological Discoveries.

C. L. HERRICK. *Journal of Comparative Neurology*, Vol. VII., Nos. 3 and 4, March, 1898. Pp. 155-161.

This brief but very suggestive article by President Herrick (University of New Mexico) adds new strength to the modern doctrines of psycho-physical relation by means of the yet more recent theory of the unity of cerebral action. Four topics are discussed in the seven pages, the first being, 'An Organ of Consciousness.' Postulating that the dynamic notion of consciousness is the only one acceptable, he contends that the doctrine of neural or cerebral equilibrium is the one to which all the neurology of the last few years has been tending, that is, that the brain is "adapted to react as a unit, though not as an invariable unit." The psychic aspect is, metaphysically, well expressed in this passage: "Pure energy with the attribute of spontaneity it could only be if it were in the mode of absolute equilibrium, in which its activities should be wholly reflected into themselves. This can only be predicated of infinite essence, and it is necessary to substitute the conditions of *relative* equilibrium in a sphere of interfering activities," the balance of nervous force in the brain being in continual disturbance from the many and varying streams of energy arriving continually from the sensory end-organs. The character of the conscious act, on the other hand ("and the elements of consciousness are always acts"), depends upon the relative extent of associational activity by the method of circular diffusion, a conscious act being always a disturbance of cerebral equilibration.

The second topic is a corollary of this theory, and is, namely: 'Neural Interpolation.' Here President Herrick refers to the temporary or permanent clusters of nerve-cells with proliferative function which Professor His has demonstrated in the olives and nuclei of the medulla and he himself in the cerebellum. These cells, by gradually taking the place of the original cells, alone make memory possible,

as the neurocytes are in a constant state of degeneration. This notion of gradual assimilation and substitution explains how, by gradual loss of the finer dendrites, the earlier and less highly correlated memories are the last to be lost.

The next 'corollary' discusses his 'Summation-Irradiation Theory of Pain-Pleasure,' first outlined in the *Journal of Comparative Neurology*, Vol. I., No. 1. "Purely physical pains have the common characteristic of involving a disproportionateness of stimulus to the conveying power of the organ," summation being an important element of the causation of pain. The interval of time between the sensation of touch and that of pain, often experienced, is, he thinks, the period required for the relaxation of the local capillaries, congestion being especially adapted to produce summation. He seems to think that this theory precludes the need of considering the now probable special nerves for pain. Pleasure, on the contrary, depends on irradiation, the most pleasurable parts of the body being those having the closest reticulum of neural fibres, as Dogiel has shown. This notion applies equally well to the higher cognitive pleasures, the irradiation then taking place in the cerebrum.

The final topic of the paper relates to the 'Dynamic Character of Consciousness,' and explains that it does not depend on the concomitant reaction of certain cerebral cells, but that it is always accompanied by a general 'refluent wave' downward, as typified by simpler reflex action. The modern accepted theories of emotion and of will make for the support of this theory, and it for the support of them. The act in every such case is a *product* of the internal cerebral conditions into the incoming currents of stimuli.

On the whole, this article is a clear and concise statement of a doctrine of consciousness in its concomitant relations which, from the researches of recent years, has not a little to say for itself. It contains more 'food for thought' than most of the discourses, so fashionable at present, many times as long.

GEORGE V. DEARBORN.

HARVARD PSYCHOLOGICAL LABORATORY.

Bibliography of Child Study. L. N. WILSON. Rep. from Ped. Sem. Worcester, Mass., Clark Univ. Press. April, 1898. Pp. 49. 50 cents.

An able and useful compilation in four parts: 1. Child Study Bibliography, of 535 titles; 2. Journals, titles 535-546; 3. Reports, Serial Studies and Transactions, titles 547-562; 4. Works of Stand-

ard Reference on Allied Topics, titles 563-641. A classified 'Subject Index' is appended which sorts the titles under headings. This, however, seems to be very inadequately done. In future issues this feature might be extended with profit.

J. M. B.

L'Année Sociologique. E. DURKHEIM. 1^{re} Année, 1896-7. Paris, Alcan, 1898. Pp. 563. 10 fr.

In this volume we have another of the very useful *Années* which are springing up in France in various departments of knowledge. The editor, M. Durkheim, well known as one of the most prominent French sociologists, has the collaboration of such authors as MM. Simmel (Berlin), Levy (Toulouse), Bouglé (Montpellier, etc.). Besides two principal articles, *La prohibition de l'inceste*, by Durkheim, and *Comment les formes sociales se maintiennent*, by Simmel, the volume contains classified notices of the literature of all countries from July, '96, to June, '97. The article by Simmel is of especial importance,¹ and we hope to return to it. The *Année* follows the plan of the *Année Psychologique* in joining original articles to the reports of literature—a plan which lacks all justification, it seems to us. The volume does not give an exhaustive *Index* of publications, but indexes only those which it reviews.

J. M. B.

L'Année Philosophique. F. PILLON. 8^{me} Année, 1897. Paris, Alcan, 1898. Pp. 312.

In this volume M. Pillon continues his annual report on the literature of philosophy, including psychology, in France for the year 1897. The leading articles are by MM. Renouvier (*De l'Idée de Dieu*), Dauriac and Pillon. M. Pillon has also recently published (Alcan, 1898) an admirable little volume on *La Philosophie de Charles Ségrétan*.

J. M. B.

A Study of Ethical Principles. JAMES SETH. Third edition, revised and enlarged. New York, Scribners (imported). 1898. Pp. xvi + 470. \$2.00.

In this edition Professor Seth has included two new chapters, one on the 'Method of Ethics' and one on 'Moral Progress.' Among the other changes the author points out that "the discussion of the place of pleasure, psychological and ethical, has been carried further." The usefulness of the book is much enhanced by the select lists of authorities cited at the end of the successive chapters, and also by the addition of an index.

J. M. B.

¹ A translation of it has appeared in the *American Journal of Sociology*, March and May, 1898.

NEW BOOKS.

Beiträge zur Akustik und Musikwissenschaft. Heft I., Konsonanz und Dissonanz. CARL STUMPF. Leipzig, J. A. Barth. 1898. Pp. vi+108. M. 3.60.

Abhandlungen zur Physiologie der Gesichtsempfindungen. J. VON KRIES. Leipzig, J. A. Barth, 1897. Vol. I., Pp. vi+198. M. 5.

Névroses et idées fixes. PIERRE JANET. Paris, Alcan. 1898. Pp. 492. Fr. 12.

L'enseignement intégral. ALEXIS BERTRAND. Paris, Alcan. 1898. Pp. 313.

John Stuart Mill, correspondance inédite avec Gustave d'Eichthal. EUGÈNE D'EICHTHAL. Paris, Alcan. 1898. Pp. xvii+238.

Dynamic Idealism. ALFRED H. LLOYD. Chicago, A. C. McClurg & Co. 1898. Pp. x+248. \$1.00.

An Outline of Christian Theology. WILLIAM NEWTON CLARKE. New York, Scribner's. 1898. Pp. ix+488. \$2.50.

Studies of Good and Evil. J. ROYCE. New York, D. Appleton & Co. 1898. Pp. xvii+384.

Vorlesungen über die Menschen- und Thierseele. W. WUNDT. 3^{te} umgearb. Auflage. Hamburg, Voss. 1897. Pp. xii+519. M. 12.

The History of the Principle of Sufficient Reason. W. M. URBAN. Princeton Contributions to Philosophy, edited by A. T. ORMOND. Vol. I., No. 1. Princeton Univ. Press. 1898. Pp. 88. 50 cts.

A Course in Experimental Psychology. E. C. SANFORD. Part I.: Sensation and Perception. Boston, Heath. 1898. Pp. vi+449.

Les lois sociales. G. TARDE. Paris, Alcan. 1898. Pp. 172. 2 fr. 50.

The First Philosophers of Greece. ARTHUR FAIRBANKS. New York, Scribner's. 1898. Pp. xvii+299. \$2.00.

Il Metodo Deduttive come Strumento di Ricerca. G. VIALATI. Turino, Frossati. 1898. Pp. 44.

The Meaning and Function of Thought-Connectives. G. T. OWEN. Rep. from Trans. Wisc. Acad. of Sciences. XII. Issued March, 1898. Pp. 48.

The Essential Differences between Man and other Animals. S. G. MEZES. Texas Academy of Sciences (reprint), read May 69 1898. Pp. 23-37.

Il lettore del pensiero John Dalton. G. GUICCIARDI e G. C. FERRARI. Estr. Riv. Sper. di Fren., XXIV., Fasc. I. 1898.

Richerche ergografiche nella donna. G. C. FERRARI. Estr. Riv. Sper. di Fren. XXIV., Fasc. I. 1898.

L'Art et le Réel. J. PÉRÈS. Paris, Alcan. 1898. Pp. 208. 3 fr. 50.

Essai sur la classification des sciences. E. GOBLOT. Paris, Alcan. 1898. Pp. 296. 5 fr.

Suggestion und hypnose. TH. LIPPS. Aus Sitzungsber. d. bayer. Akad. d. Wiss., 1897. München, Straub. 1898. Pp. 391-522.

NOTES.

CHARLES H. JUDD, Ph.D., of Wesleyan University, has been called to the chair of physiological and experimental psychology in the School of Pedagogy, New York University.

RAYMOND DODGE, Ph.D. (Halle), of Ursinus College, will succeed Dr. Judd as instructor in philosophy at Wesleyan University.

DR. DICKINSON S. MILLER has been appointed lecturer in psychology in Columbia University.

EDWARD THORNDIKE, Ph.D. (Columbia), has been appointed instructor in psychology in the Western Reserve University.

F. KENNEDY, Ph.D., of Princeton, has been appointed assistant professor of philosophy in the University of Colorado.

DR. C. M. BAKEWELL, of the University of California, has been appointed associate professor of philosophy at Bryn Mawr College. Dr. James H. Leuba, who was elected a year ago associate in psychology and pedagogy at Bryn Mawr, will begin his courses next year. The fifth floor of Dalton Hall is being adapted to the requirements of a psychological laboratory, and the necessary apparatus is being procured.

DR. JOHN BIGHAM has retired from the chair of philosophy in De Pauw University.

F. E. BOLTON, who will receive a degree of doctor of philosophy from Clark University, has been elected a grammar school principal by the school board of Springfield, Mass.

THE Curators of Edinburgh University have unanimously appointed, as successor to the late Professor Calderwood in the chair of moral philosophy, Professor James Seth, M.A. (Edinburgh), Sage professor of moral philosophy in Cornell University.

THE University of Dublin has elected to the chair of mental and moral philosophy Mr. Swift Paine Johnston, who is said to be an American citizen.

MR. HENRY WILDE, F.R.S., has endowed in Oxford University a readership and a scholarship in mental philosophy. They are to be designated the Wilde readership and the John Locke scholarship.

THE Open Court Publishing Company has issued the first two portfolios of its series of portraits of philosophers and psychologists. The first installment of philosophers includes Bacon, Hobbes, Descartes, Spinoza, Locke, Hume, Condillac and Kant. The psychologists are Cabanis, Taine, Hering, Mach, Ladd, Hall, Stumpf and Morgan. The portraits are excellently executed, and are sold at such a low price that they should be in the hands of all students of philosophy and psychology.

THE publication of a monthly *Rivista di scienze biologiche* under the editorship of Professor Enrico Morselli is announced. It proposes to cover somewhat the same field as the *American Naturalist* and *Natural Science*, and has the coöperation in England of Sir John Lubbock and in America of Professor J. Mark Baldwin. Subscriptions may be addressed to Dr. Paulo Celesia, via Assarotte 46, Genoa.

